A

JOURNAL

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CHEMISTRY,

AND

THE ARTS.

VOL. I.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE conclusion of the first Volume of the new Series of this Journal, demands that I should return my sincerest acknowledgments to those excellent Correspondents who have assisted me in improving it, and to the Public at large, who have shewn by the increased demand, that they approve the alterations which have been made.

Among the advantages I will here mention only two; the first by no means inconsiderable, is the very neat and correct typographical execution, which would be honorable to the Printer of a complete copy of a work at leisure, and is singularly valuable in a periodical work;—the other, namely the side notes, requires so much diligence and expence, that it is likely they will long continue to be peculiar to this Journal. These circumstances, added to the unvaried fidelity of conduct in quotation and regard to the rights of others; from which the Editor never has nor will depart, have naturally led men of eminence to chuse this Journal as the repository for their communications.

It is with a very high degree of satisfaction that I can state, that half the materials contained in the present Volume are original, and avowed by their very respectable Authors; that one third part confifts of translations, or abstracts of excellent works never before publifhed in English, and that the remaining sixth part are select Memoirs, either abridged or extracted from the Transactions of the Royal Society, and other recent and authentic sources of discovery.

The Authors of original Papers are Doctors Thomson, Beddoes, Higgins, and Prieftley; the Rev. W. Pearson; Messrs. Gough, Close, Davy, Murray, R. L. Edgeworth, Stodart, Zach, Accum, Ez. Walker, Goodwyn, W. Walker, and W. N.—Of foreign works, Thenard, Peres, Tromsdorf, Prevost, Dolomieu, Proust, Loysel, Hauy, Volta, Van Marum, Le Blanc, Carradori, Darcet, Parmentier, Vauquelin, Darracq.—And of English Memoirs abridged or extracted, Mendoza, Herschel, Chenevix, Davy, Ware,

Cooper

Cooper, Englefield, Delafons, Sheldrake, Capper, Arkwright. For the various and important objects of their research, he must refer to the Table of Contents, the ample Index, and the other assistant indications he has been sedulous to present to the Reader.

This Volume contains sixteen Copper Plates, illustrating nineteen distinct objects: viz. Herschel's discoveries of the Structure and Changes of the Sun. 2. Various circular Instruments for measuring Angles, 3, 4. Two Hydraulic Engines invented by Mr. Close for raising water above its Level by the Syphon. 5. Method of making Gun Flints. 6. Mr. Cooper's Designs to shew by the structure of the Ear, that a species of Deafnefs may be cured by puncture of the Membrane of the Tympanum. 7. Loysel's new Apparatus for bleaching Paper. 8. Carangeau's Instrument for measuring Crystals. 9. Dr. Thomson's Apparatus to shew that the Rumfordian Circulation of Solids in Fluids is not occasioned by Currents. 10. Volta's Sketches in support of his Theory of Galvanism. 11. Trevithack's new and powerful Engine for producing Mechanic Force by an included Column of Water. 12. Mr. Murray's Apparatus for new Experiments on the conducting Power of Fluids. 13. Dr. Herschel's Apparatus for viewing the Sun through Fluids. 14. Mr. Hornblower's Hydraulic Bellows for a Forge. 15. Rev. Wm. Pearfon's Projection of the Orbits of the Earth and of the new Planet Ceres, 16. Mr. Murray's Apparatus for shewing the conducting Power of Mercury and of Oil in a Veffel of Ice. 17. A simple and free Efcapement for Time Pieces, by Mr. Delafons. 18. Mr. Arkwright's Engine for raising Ores from Mines. And, 19. A cheap, simple, and accurate perspective Instrument, by Richard Lovell Edgeworth, Esq.

With prospects already fo animating, under the favorable circumstances of a general Peace, and the facilities it affords to philosophical pursuits and manufacturing inventions, it is to be expected that the value of our communications and discoveries will increase.

Soho Square, London, May 1, 1802.

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ERRATA.

I have obferved no Errata by which the Reader can be mifled, but muft remark, that the table at page 73, called *Obfervations* of the Ceres is wrong intitled: the table being merely an ephemeris, *calculated* for re-difcovering the planet from the former obfervations of Piazzi.

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JANUARY, 1802.

ARTICLE I.

On the Exhibition of a Series of Primes, and the Refolution of a Compound Number into all its Factors. By Mr. J. GOUGH.

To Mr. NICHOLSON.

SIR,

M. EULER remarks in his Algebra, that mathematicians have not yet difcovered a way of exhibiting a natural feries of primes; and in reality I know of no writer on arithmetic, who fhews how a compound number may be refolved into all its factors, otherwife than by trial. The following method, however, may be faid to folve both problems, perhaps in as ready a way as the nature of numbers will admit; and on fixed principles: it depends upon the following property of compound numbers.

Natural feries of primes and refolution of a number into its factors not yet effected.

If a compound number, which is not a fquare, be refolved into any two of its factors, one of thefe factors will be lefs than the root of the next greater fquare, and the other will be equal to, or greater than the fame root.—If you deny the property, then both factors muft be either greater than the root of the fquare, in which cafe the product will be greater than the fquare; or, they muft be both lefs, than that root, on which

Property of compound numbers.

ſuppoſition the product will not be greater than the next leſs ſquare, ſeeing the factors are whole numbers. Now ſuppoſe f^2-c to repreſent any number whatever, where f^2 is the next greater ſquare, and c the exceſs of that ſquare above the given number: then any two factors of f^2-c may be expreſſed by $f-p$, and $f+p+2n$, from whence the following method of determining all the factors to two ſucceſſive ſquares, and their intermediate numbers, is eaſily derived.

Method of determining factors.

In a perpendicular column at the left hand of your future work, (*vide Tab.* 1ſt.) which exhibits the diviſors of all the numbers from 49 to 36, write down the arithmetic progreſſion 1, 2, 3, &c. beginning at the top with unity, and ending with f; let all theſe numbers ſucceſſively repreſent the diviſor $f-p$, and correſponding to each of them, draw a double horizontal column: when $f-p=1$, make c in the uppermoſt horizontal column $=2f-1$, $3f-2$, $2f-3$, &c. to 0, becauſe $2f-1$ is the difference of the two ſquares in all caſes; and for $f+p+2n$, write $\overline{f-1}|^2$, $\overline{f-1}|^2+1$, $\overline{f-1}|^2+2$, &c. to f^2. In order to fill up the other columns, divide f^2 by the reſpective value of $f-p$, and place the remainder, if there be any, in the upper part of the horizontal column, perpendicularly under the ſame value of c in the higheſt column; but if there be no remainder, place a cypher in its room: then increaſe this remainder as often as you can by the addition of $f-p$, not to make it exceed $2f-1$, and place theſe, each directly under the ſame number in the higheſt column, and write c on the left hand. Under the firſt remainder or cypher, place the quotient $\frac{f^2}{f-p}$ in whole numbers; from which, ſubtract unity as oft as $f-p$ was added to the remainder, and call theſe $f+p+2n$; but when $f-p=f$, put 0 alone for the value of c: when all the columns are filled up in this manner, the factors of any given number are thus found. Find the value of c correſponding to that number in the higheſt column; then find how often the ſame number recurs, by caſting your eye perpendicularly down the columns, the factors will be the value of $f+p+2n$, directly under that value of c in each column; and the correſponding value of $f-p$: conſequently, theſe values of c which are only found in the higheſt column, indicate that the number

number directly under it is a prime, as is the case in *Tab.* 1st, with 37, 41, 43, 47.

But if a series of primes only is to be calculated, the work may be shortened, by omitting the values of $f+p+2n$ in all the columns except the highest, and marking those numbers with asterisks which correspond to the values of c only found in the uppermost line, as in *Tab.* 2d. where the prime numbers from 1 to 16 are calculated. **Primes.**

Note, the values of c may be otherwise calculated:—from the product fp take the greatest multiple of $f-p$ that can be subtracted from it; this will give the least value of c corresponding to that value of $f-p$.

Your's, &c.

Kendal, Dec. 7, 1801. J. GOUGH.

TABLE I.

$f^2 = 49$

$f-p=1$	$c=$	13	12	11	10	9	8	7	6	5	4	3	2	1	0
	$f+p+2n=$	36	37	38	39	40	41	42	43	44	45	46	47	48	49
$f-p=2$	$c=$	13		11		9		7		5		3		1	
	$f+p+2n$	18		19		20		21		22		23		24	
$f-p=3$	$c=$	13			10			7			4			1	
	$f+p+2n$	12			13			14			15			16	
$f-p=4$	$c=$	13				9				5				1	
	$f+p+2n$	9				10				11				12	
$f-p=5$	$c=$					9					4				
	$f+p+2n$					8					9				
$f-p=6$	$c=$	13						7						1	
	$f+p+2n$	6						7						8	
$f-p=7$	$c=$														0
	$f+p+2n$														7

TABLE II.

		From 1 to 4				From 5 to 9					From 10 to 16						
$f-p=1$	$c=$	3	2	1	0	4	3	2	1	0	6	5	4	3	2	1	0
	$f+p+2n$	* 1	* 2	* 3	4	* 5	6	* 7	8	9	10	* 11	12	* 13	14	15	16
$f-p=2$	$c=$				0		3		1		6		4		2		0
$f-p=3$	$c=$									0			4			1	
$f-p=4$	$c=$																0

II.

On an improved Reflecting Circle. By JOSEPH DE MENDOZA RIOS, *Esq. F. R. S.* (*Phil. Transf.* 1801.)

Advantages of large instruments in diminishing the errors of division and eccentricity.

IN practical astronomy large instruments are useful, not only to enable the observer to read the angles to a small fraction of a degree, but likewise to diminish, in the construction, the inaccuracies which proceed both from the errors of the divisions and the eccentricity of the index. Frames of considerable dimensions admit also the application of telescopes with great magnifying powers, which is a circumstance of the utmost importance in celestial observations. As the reflecting instruments employed at sea are supported by the hand, their weight and scale are limited within a narrow compass; and it seemed very difficult to obviate, by any expedient, the inconveniences arising from the smallness of their size, while it was impossible to increase it. The celebrated Tobias Mayer contrived, however, a method to determine, at one reading, instead of the simple angle observed, a multiple of the same angle; and, by this means, the instrument became, in practice, capable of any degree of accuracy, as far as regards the above mentioned errors. His invention is essentially different from the mere repetition of the observations; and my object requires that I should explain the principle upon which it is founded.

Invention of Mayer for diminishing the same errors at pleasure in small instruments by reflection.

Mr. Mayer propofed to complete the limb of the fextant, making a whole circle, with the horizon glafs moveable round the centre, with an additional index, which I fhall call *the horizon index*, in order to diftinguifh it from *the centre index*, to which the centre glafs is attached. This inftrument is reprefented in Plate I. Fig. 1; and the manner of ufing it is as follows: after the index A is fet at o, (the beginning of the divifions,) the two glaffes are rendered parallel, as is ufually practifed with Hadley's quadrant, by moving the horizon index B, till the horizon of the fea, (or the fun, or any other object,) or its direct image, and the doubly reflected image of the fame, feen through the telefcope, coincide. After fixing the horizon index in that pofition, the centre index A is to be moved, in order to meafure the diftance of the two objects S and L, (which I fhall fuppofe the fun and moon,) by bringing into contact the doubly reflected image of the fun with the direct image of the moon, feen through the telefcope. The centre image will then be at M, and the arch o M might give, as in the fextant, the angular diftance required; but the conftruction of the circle renders it eafy, in this pofition, to effect again the parallelifm of the glaffes, and to make another obfervation of the contact, in the like manner as from o; which operation will bring the centre index to N. The index will then give o N, or double the diftance; and, as it muft be divided by 2, in order to have the angle required, the errors of divifion and eccentricity, which, together, I fhall call the *error of the inftrument*, will be likewife reduced to one half. It is obvious, that by fucceffive repetitions of the fame procefs, triple, quadruple, &c. the diftance may be obtained, and the faid error further reduced, in the inverfe ratio of the multiplication of the diftance, to any degree of approximation required.

1. He made the limb an entire circle. And 2. he fixed the horizon glafs upon an index moveable on the fame centre, but independant of that which carries the glafs called the index glafs.

Ufe. Adjuft the glaffes parallel by the horizon index.

Obferve the angle by the firft index and fix it.

Make the glaffes again parallel by the horizon index.

Obferve the angle as before.

Adjuft for parallelifm, and repeat the obfervation.

And as this procefs may be continued at pleafure, and the firft index will pafs over the meafure of the angle as many times as there are repetitions; Divide the whole fpace by the number of repetitions, and the quotient will be the correct angle; the errors of the inftrument being diminifhed in the fame ratio.

The method of rendering the glaffes parallel, by means of the horizon of the fea, is not accurate, on account of the indiftinctnefs of the images; and, when the fun is ufed for that purpofe, the obfervation becomes fatiguing to the eye. The repetition of that obfervation, by one or the other method, remained therefore a confiderable inconvenience attached to Mr. Mayer's circle. The author himfelf feems to have been of that opinion, as he propofed to provide the inftrument with a diagonal rule, fixed upon one of the indexes, fo that the

other

Objection. It is inconvenient to make the adjuftments for parallelifm. Happy invention of Borda for removing it.

other index fhould touch it when the glaffes were parallel; but an adjuftment of this nature muft be fubject to great errors, and was never adopted in practice. The Chevalier de Borda, wifhing to remove that imperfection, had the happy idea of rendering the parallelifm of the glaffes unneceffary, by fubftituting the obfervation of the angular diftance of the two objects, to that of the coincidence of the images of the fame object. This conftitutes the fecond great improvement of the reflecting circle, which it is neceffary for me to explain, before I proceed to the account of my own inveftigations *.

By a flight alteration in the conftruction he renders it practicable to obferve the fame angle twice, by moving the index in contrary directions without fhifting the face of the inftrument;

In Borda's circle, the telefcope is fixed at fome diftance from the centre, and the horizon glafs is carried near the border of the inftrument, as in Plate I. Fig. 2. By this arrangement, the rays of light can arrive at the centre glafs, both from the heavenly bodies fituated to the right of the horizon index, as S′, and from thofe fituated to the left, as S. Thus, if the glaffes are parallel to one another, when the centre index is at o, it is obvious that there are two ways of making the obfervations. While the direct image of the moon L is feen through the telefcope, the angular diftance to the fun, if at S, may be meafured by moving the centre index to m, in order to produce the contact; or, if the fun is at S′, the fame operation may be performed, by ufing the contrary motion to m'. The firft kind of obfervation, the Chevalier de Borda calls *obfervation to the left*; and the fecond, *obfervation to the right*. Suppofe now, that (the horizon index being fixed in the fame pofition) the diftance from L to S is obferved to the left, by bringing into contact the doubly reflected image of S with the image of L, feen without reflection; let us then turn the inftrument round, keeping it in the fame plane, fo as to have the direct image of S through the telefcope, and thus make an obfervation of the fame diftance to the right; the pofition of the centre index being in the firft obfervation at m, and in the fecond obfervation at m', it is clear, that if o is the point where the parallelifm of the glaffes takes place, om is equal to om'; and, that the arch $m\,m'$, determined by the two pofitions, will give double the diftance.

which gives the double diftance, and requires not any adjuftment for parallelifm.

* It does not belong to my prefent plan, to explain the effect of Borda's improvement in correcting the errors which arife from the want of parallelifm in the furfaces of the glaffes. This will be fully confidered in another Paper, where I intend to give an account of feveral inveftigations which I have made upon that particular fubject.

It

Convenient manipulation.

It will be more convenient to have the centre index at o, when the first observation is made, in order to take the double distance at one reading, after the second observation. For this purpose, the first part of the process may be inverted, by previously fixing the centre index at the beginning of the divisions, and moving the horizon index H towards o, instead of moving the centre index A to *m*, or towards H.

This is denominated the *crossed observation.*

The last kind of observation, in which the incident ray, which produces the first image upon the centre glass, may be conceived to run double the angular distance, passing in its way over the line of collimation, has been called, by the Chevalier de Borda, *the crossed observation.*

It may be repeated at pleasure.

The same process may be repeated, by fixing alternately one of the indexes, and moving the other, and continuing successive sets of observations; each set of two crossed observations, one to the right and another to the left. The angle given by the instrument, will be equal to double the angular distance multiplied by the number of sets observed, or, in other terms, to the angular distance multiplied by the number of observations, which are always supposed to be made by pairs; an odd observation being of no value in this manner of using the circle.

And will not require a direct view of the brighter object, if the plane of the instrument be reversed at the second observation.

I have expressed myself as if the observations could always be made by looking alternately at each object through the telescope, in order to bring into contact the doubly reflected image of the other object. This is not the case in the observations of the distances from the moon to the sun, or a star; it being then indispensable to compare, by reflection, the brightest of the two heavenly bodies; but there is a very easy method of obviating that inconvenience. After the contact of the images of S and L is observed, with the telescope directed to L, the position of the plane of the instrument may be inverted, turning it round the axis of vision O B L; the incident ray will then answer to the point S′, equally distant from L as S, and the crossed observations will still give S S′, or double the distance.

Whether a circle is used simply, as Mayer proposed it, or according to Borda's method, its peculiar advantages chiefly depend on the multiplication of the distance required. I have therefore turned my attention to the improvement of this principle; and, with that view, I have contrived the construction which I am going to describe.

In

The author's improvement consists in applying an entire moveable circle, *called the flying nonius*, within or upon the divided circle. Each of these circles may be attached to either the center index or horizon index by the pressure of a clamp.

In the crossed observations made with Borda's circle, the indexes move alternately through an arch which in the divisions, is equal to double the distance: for example, the centre index comes, in the first crossed observation, from m to m'; in the third crossed observation, from m' to m'', &c. and the horizon index, in the second crossed observation, to h': in the fourth crossed observation, to h'', &c. and, by each of the two indexes may be found the same multiple of the distance required. Let us now place the Nonius in a circle moving round the centre, over, or adjacent to, the usual limb which contains the divisions: it will easily be conceived. that, by attaching that circle, which I shall call the *Flying Nonius*, alternately to each of the indexes, it will serve as Nonius for both; and that, after any number of observations, it will give the compound motion of the two indexes. Thus, after the first observation, the Flying Nonius will, at each crossed observation, advance double the distance over the divisions, while each separate Nonius, fixed on the indexes, requires a set of two observations, to produce the same effect in Borda's circle.

View and description of the *improved reflecting circle.*

Plate I. (XXX. XXXI. and XXXII.) exhibits a perspective view, (a plan, and a section,) of the instrument, which, for the sake of distinction, I shall call my *Improved Reflecting Circle.* (The last Plate is particularly intended to shew the compound handle, which I have adapted to the instrument, in order to hold it with convenience and ease in every position *). These three

* The use of Mayer's circle, or of Borda's, as constructed till now, with only one handle attached to the centre, is extremely inconvenient in several positions, and particularly when it must be kept inverted downwards during the observation. For this reason, I thought it of importance to contrive such a support as would enable the observer to hold the instrument with the same ease in every direction. This is effected by means of the compound handle, attached to the horizon index, by the brace V and screw X, (Plates XXX. XXXI. and XXXII.) which index turns round the centre with the handle. When Mr. Troughton began to construct this sort of instrument, I recommended to him this improvement, which he has adapted to his reflecting circle.

I shall observe here, that Mr. Troughton's circles are not of the kind which I have endeavoured to improve. The scheme of his construction may be said to consist in completing the limb of a sextant

three repreſentations, in which the ſame parts are marked with the ſame letters, are ſufficient to give an accurate idea of the arrangement of the whole, and make it unneceſſary for me to enter now into a minute detail of the mechaniſm of the apparatus. I therefore ſhall content myſelf with adding here only what concerns the general uſe, and the peculiar properties, of this inſtrument.

M is the divided limb of the circle, and N the Flying Nonius, to each of which the horizon index may be occaſionally attached, by means of the clamps D, C; as well as the centre index, by means of the other clamps A, B. The peculiar property of the inſtrument being that of giving double the diſtance, I have thought proper to divide the circle into 360 degrees, and not into 720 according to the nature of the ſextant. Thus, after a croſſed obſervation, the reading of the Nonius will, without reduction, exhibit the meaſure of the ſimple diſtance. I have likewiſe extended the nonius round the circumference, ſo that, by the coincidence of two diviſions, the number of degrees will appear on the limb, and that of the minutes and ſeconds on the flying nonius. The manner of making the obſervations with this inſtrument is as follows:

The circle is divided into whole degrees, and the flying nonius has its diviſions extended all round its circle.

Adapt the o of the flying nonius to 360° of the limb; and then faſten the two clamps A, B, of the centre index * E E, by which the diviſions will be kept in the ſame relative ſituations. Then, tnrn round the horizon index F F, and make an obſervation of the diſtance to the right. The contact muſt be adjuſted by the ſcrew G, the clamp C being faſtened.

Method of obſerving. Clamp the center index both to the limb and nonius (ſet with o oppoſite 360°). Obſerve an angle to the right by moving the horizon index; which then clamp to the nonius. Diſengage the center index from the nonius, and repeat the obſervation to the left: the flying nonius will give the diſtance.

tant to the whole circumference, and making it capable thereby of performing Borda's croſſed obſervations, with as many noniuſes as may be attached to the centre glaſs. But, Mr. Troughton's inſtrument is deprived of the principle invented by Mayer, for obtaining at one reading a multiple of the diſtance required; which is the great property of circles, and, in my opinion, the beſt means of diminiſhing their errors.

* Properly ſpeaking, neither the centre nor the horizon indexes act, in this inſtrument, as ſuch, both of them being deprived of the nonius, which is transferred to the flying circle; but, for the ſake of perſpicuity, I continue the uſe of thoſe expreſſions, in order to diſtinguiſh the plates or rules which carry the centre and the horizon glaſſes.

Leave

Leave this clamp faftened, and loofen the clamp A; thus turn the inftrument, and make a croffed obfervation to the left, adjufting by means of the fcrew H, after having faftened the clamp D. At the end of this obfervation, the flying nonius will give the diftance. Faften now the clamp A, and loofen the clamps B and C, leaving the clamp D faftened; then turn the inftrument again, and make a croffed obfervation to the right. At the end of this obfervation, the flying nonius will give double the diftance. By fucceffively inverting the ufe of the clamps, this alternate procefs may be continued *ad libitum*; and each croffed obfervation will increafe the reading, by an arch equal to the diftance.

Remarks. Let the number of obfervations be n, and the angular diftance D. The arch given by my improved circle will be $= D\ (n-1)$. In Borda's circle, (reducing the divifions of the fextant to thofe of the theodolite I ufe,) the arch is $= D \times \frac{1}{2}\, n$; and, either n muft be an even number, or the odd obfervation muft be loft. In Mayer's circle, the arch is $= \frac{1}{2} D \times \frac{1}{2}\, n$; and the number n, which comprehends the obfervations for the parallelifm of the glaffes and thofe for the diftance, muft likewife be even. The comparifon of thefe expreffions, fhews at once the relative advantages of the different inftruments.

My conftruction offers confiderable advantages, in every manner of ufing the circle. If, inftead of the croffed obfervations, it fhould be wanted to employ the ufual practice of rendering the glaffes parallel, a multiple of the diftance may ftill be obtained by my inftrument, equal to that of the other method. For this purpofe, the parallelifm of the glaffes may be effected, by means of the images of the fun, or the horizon of the fea, moving the index F, while the o of the nonius is adapted to 360° of the limb, and the two clamps A, B, are faftened. After this, an obfervation of the diftance to the right may be made, with the clamp A faftened, while the clamp B is loofe; the clamp D being alfo faftened, and the clamp C loofe; and, at the end of this obfervation, the flying nonius will give an angle, which will be only the half of the diftance in my divifions, but which would be equal to the whole diftance, if the divifions were according to the fextant. After that, and while the clamps B and C are faftened, and the clamps A and D loofe, the parallelifm of the glaffes may be

be again effected; and the nonius will advance the fame quantity over the limb. The fame addition will take place, by inverting the ufe of the clamps, and making another obfervation of the diftance. The like alternate procefs may be continued indefinitely; and the refult given by the inftrument will, with only one obfervation more, be the fame as that of Borda's method, and double the arch which would be obtained by Mayer's circle.

Mr. Borda's circle is liable to a very great inconvenience in practice. Each index advances fucceffively over the limb; and, in order to facilitate the operation of bringing the images for the contact within the telefcope, that author advifes to make a preparatory memorandum of the pofitions which the indexes will nearly occupy, fo that they may be fet accordingly, previous to each obfervation. But this method, which is always inconvenient, by night becomes almoft impoffible. For this reafon, I have joined to the horizon index an arch L L, (Plates XXX. and XXXI.) which is divided, both to the right and left, into degrees and minutes of the fextant, fo that, when the glaffes are parallel, the centre index coincides with the two firft divifions o, o, and occupies the fpace left blank between them. I have further provided two fliding pieces P, P, which may be adapted to that arch, with a fpring fufficient to keep them firm in any fituation. Putting each of thefe pieces upon the arch, fo that their ends may coincide with the divifions marking the rough diftance to be meafured, no more will be required, than to fet the centre index alternately againft each piece, before the beginning of the fucceffive croffed obfervation. The clamp may then be faftened, and the remainder of the motion produced by the adjufting fcrew; as, if neceffary, the index will pufh the fliding piece further, and leave it at the point where the contact was effected *.

Eafy contrivance for fetting to the diftance nearly, before each obfervation.

The flying circle facilitates the ufe of any number of noniufes, which may be applied round the whole circumference; but, as the leading principle which I have chiefly had in contemplation, is that of obtaining an accurate refult from one reading, I have only ufed a fingle nonius. Two noniufes,

Other advantages, fuch as the increafed number of noniufes, if required.

* The idea of this fimple contrivance, was fuggefted to me by the ingenious Mr. E. Troughton.

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opposite one another, might however be advantageous, in order to correct the errors of eccentricity; but, in my opinion, a greater number ought not, in any case, to be used.

It may be used in circles where the telescope is attached.

Before I conclude this Paper, I shall remark, that my improvement may be partially applied to a circle, where the telescope and the horizon glass are attached, or fixed, to the main frame of the instrument. The flying nonius, acting then with the single centre index, will only give the same result as Borda's circle; but this construction seems to me greatly preferable to all the other plans executed till now; the whole apparatus being more solid and simple, and its use not liable to the errors which arise from the motion of the horizon index.

It affords a method of repeating observations beginning from a stop.

With this construction, we may likewise employ a method of ascertaining the place where the paralleiism of the glasses was observed to take place, and of setting the index afterwards in the same situation, as often as is necessary for the repetition of the observations. A piece may be used, so contrived as to be attached to, or detached from, one side of the index, by means of a screw; and provided besides with other screws, to fasten it to any part of the limb. This rectification piece, being previously attached to, and carried with, the index, must be fixed in the situation it occupied when the contact of the images was observed. The index will then be detached from it, in order to observe the distance, and afterwards must be brought back to the same position as before, contiguous to the rectification piece. The like alternate process may be repeated; and the flying nonius, going with the index in the motions forwards, and standing still in the motions backwards, will give the multiple of the observed angle, without performing the observation for the parallelism more than once in the beginning. In Mayer's circle, as well as in Borda's, there is a great objection to any attempt for that purpose; because, as the horizon glass moves round with the index, its perpendicular position is deranged by the inequalities of the plane of the limb; but, in my construction for multiplying with the horizon glass fixed, that inconvenience is removed; and the method of ascertaining the identical position of the glasses may be employed in practice, with advantage, it being done when the index is at the same point of the frame. By suggesting this idea, I do not, however, mean to represent it

it as preferable to the repetition of the obſervations; which proceſs muſt, for many reaſons, have the advantage over any mechanical contrivance whatſoever.

I have procured reflecting circles to be conſtructed, upon the principles here deſcribed, both with the teleſcope and the horizon glaſs upon a moveable index, and with the ſame pieces attached to the main frame of the inſtrument. The two methods have reſpectively anſwered my expectation; and I purpoſe, at a future opportunity, to publiſh a deſcription of the means which I wiſh to recommend for the mechanical improvement of the different parts, together with an account of ſome other eſſays which I have made relative to the ſame ſubject. The invention ſucceeds in practice.

III.

Obſervations tending to inveſtigate the Nature of the Sun, in order to find the Cauſes or Symptoms of its variable Emiſſion of Light and Heat; with Remarks on the Uſe that may poſſibly be drawn from Solar Obſervations. By WILLIAM HERSCHEL, LL.D. F.R.S.*

ON a former occaſion this eminent aſtronomer gave his reaſons for inferring that the ſun is a moſt magnificent habitable globe In the preſent paper he proceeds conſiderably farther in the inveſtigation of the phyſical and planetary conſtruction of that luminary. He introduces his ſubject by remarking, that the influence of this luminous body on the globe we inhabit is ſo great and ſo widely diffuſed, that it becomes almoſt a duty for us to ſtudy the operations which are carried on at its ſurface; that from motives ariſing from the important effects of light and heat we ought certainly to examine their ſource, and though, like the Egyptians with the Nilometer, or modern philoſophers in their obſervations upon the atmoſpheric phenomena, we can entertain no hope of modifying the effective cauſes, yet, by arranging the particulars of our own conduct by the fore-knowledge of events though inevitable, we may add much to the general felicity and enjoyments of ſociety. The ſun inferred to be an habitable globe. Its influence of the greateſt importance to us, and deſerves to be ſtudied.

* Abridged from the Philoſophical Tranſactions, 1801, p. 265. W. N.

New terms of solar phenomena.

Dr. Herschel, in consequence of the improvements in his telescopes, and the advances in his knowledge of the physical construction of the sun, has rejected the old terms of *spots, nuclei, penumbræ & luculi*, and has substituted those of openings, shallows, ridges, nodules, corrugations, indentations, and pores.

The luminous matter of the sun is of the nature of clouds, or atmospherical phenomena.

In his former paper on the nature and construction of the sun and fixed stars, the author shewed that the lucid substance of the sun is not a liquid nor an elastic fluid, but that it exists in the manner of luminous clouds swimming in the transparent atmosphere of the sun, or rather of lucid decompositions taking place within that atmosphere. * His present observations confirm and extend that induction. They are arranged under certain enunciations, which I shall proceed to give, with so much of the particular facts as may be requisite for their elucidation.

Of Openings.

Openings; or places where the luminous solar clouds are removed.

Openings are places where the luminous clouds of the sun are removed, so as to exhibit the opaque globe of the sun through the aperture. On the 4th of January, 1801, there was a large opening much past the centre of the sun, with a shallow about it, as exhibited in Fig. 1, Plate II. On the preceding side the thickness of the shallow was visible from its surface downwards, but on the following side the edge of the shallow only could be seen, but not its thickness. The side of the elevation surrounding the shallow was also seen going curvedly down to the surface of the shallow on its preceding side. Fig. 2 represents a section of the same opening, in which the lines *a b c d f* are supposed to be drawn from the eye of the observer, corresponding with lines marked by the same letters in Fig. 1. The line *d* goes through the opening to the surface of the sun A B. Fig. 2 shews evidently why the thickness of the shallow and elevation of the luminous parts are seen on one side and not on the other.

Particular description.

General facts respecting the solar openings.

Large openings have generally shallows about them. Many openings are without shallows. Small openings are generally without shallows. Openings have generally ridges and nodules about them. The openings have a tendency to run into each other,

* Philos. Trans. 1795, p. 72.

other, and new ones break out near those already formed. The probable cause of openings is, that an elastic, but not luminous gas, issues up through the pores or incipient openings, and spreads itself on the luminous clouds, forcing them out of the way, and widening its passage. The direction and operation of this cause appears to be not equally extended on all sides, but is frequently oblique, so that the luminous clouds are driven, and form a larger shallow on one side. Different openings being effected similarly, seem to shew a correspondent effect or unity of direction to some extent. They are produced by a wind or gas from the sun's body.

Fig. 3 represents an opening, with a branch coming from its shallow, seen February 18, 1801. Fig. 4 represents the same opening as it appeared one hour afterwards, broken out at three places on the one side, with corresponding projections in its shallow, which is very large on the side towards which those breakings out point, but is more confined on the other quiescent side. Changes in the openings observed.

Fig. 5 represents a small oblong opening, with a very long shallow, the luminous matter being close to the opening on the other sides. Eight other small openings, forming a cluster, had each their shallows on the same side. In three hours it changed to the appearance at Fig. 6, and an hour afterwards an opening began to appear in the furthest end of the shadow, as at Fig. 7. When the shallows begin to diminish, and the lips or projections to disappear, the openings were observed to be at the period of their greatest extent. Fig. 8. There is some difference in the colour of openings, apparently from a thin veil of luminous clouds hovering above them. When the openings are decaying, they divide as in Fig. 9, in which observation, December 27, 1799, the luminous passage across the opening resembles a bridge thrown over a cave or hollow space. The opening was completely gone the following day. Openings sometimes increase after beginning to diminish. They generally grow less and vanish. They sometimes become converted into large indentations, with or without pores or small openings, and when they have vanished, they leave the surface disturbed more than common. Observations.

The depth of the openings is visibly apparent, and the distance between the shallows and the solar surface is indicated by the free motion of low clouds. Fig. 10 shows a large Observation of the depth, &c. of openings. Low moving clouds.

Narrative.

large opening obferved January 25, 1801, at $9^h\ 22^m$. The note is as follows: " A large opening, which I have been obferving fince the 19th, is now much advanced towards the limb. I can fee into it; and on the preceding fide, as it appears to me, a good way under the loweft regions of the clouds of which the fhallow confifts. The upper margin of the fhallow is very fharply determined; but the clouds at the lower part of it, on the contrary, are more difperfed; fome of them hanging a good way down towards the furface of the fun's body. See Fig. 11. At $10^h\ 20^m$ the preceding fide of the fhallow of the large opening is now more abruptly terminated at the bottom of its thicknefs; the hanging or projecting clouds being removed towards the following fide. See Fig. 12."

Of Shallows.

Shallows, or places depreffed below the luminous clouds.

Shallows are depreffed below the general furface of the fun, and are places where the luminous folar clouds of the upper regions are removed. Their thicknefs is vifible. They fometimes exift without openings in them. They begin from the openings or branch out from fhallows already formed, and go forwards. The probable caufe of fhallows is deduced from the obfervations, among which the following are delineated.

Obfervations of the fhallows;

January 25, 1801, $9^h\ 20^m$. Two branches, A B, Fig. 13, of a fhallow coming from an opening C, going towards the fouth. It feems as if they were deftined to meet the incipient fhallow of a fouth-following opening D. $9^h\ 50^m$ the fhallow B is very nearly united to the narrow part of the fhallow furrounding the opening D. The fhallow H feems to advance in a direction towards the fartheft fouth-following opening E. $10^h\ 20^m$ the fhallow B is completely run into the fhallow about D; and the fhallow A is grown broader towards F. $11^h\ 30^m$ the fhallow B is fo completely joined to the fhallow D, tbat it appears as if it had not come from the opening C. The fhallow A now ends in a fharp point, Fig. 15. $12^h\ 50^m$ the fhape of the fhallow A is no longer pointed, but very broad at the end, and there is a new branch breaking out at G, Fig. 16. Thefe changes feem all to denote that the fhallows are occafioned by fomething coming out of the openings, which, by its propelling motion, drives away the luminous

they are caufed by the propelling gas which produces the openings.

luminous clouds from the place where it meets with the leaſt reſiſtance; or which by its nature diſſolves them as it comes up to them. If it be an elaſtic gas, its levity muſt be ſuch as to make it aſcend through the inferior region of the ſolar clouds, and diffuſe itſelf among the ſuperior luminous matter. 1h 10m the new branch G increaſes, and the openings C D E are enlarged. A new branch is alſo breaking out from the ſhallow about E. It is marked H in Fig. 14, and denoted with points. Theſe changes ſeem to prove that the ſame gas which diffuſes itſelf over the ſhallows, has forced open the paſſages at firſt, and is now widening them. Hence the increaſe of the openings is an additional circumſtance which points out the cauſe of the ſhallows. 1h 20m: from the ſhallow of a very large preceding opening, which is in an increaſing ſtate, are lately projected three ſmall branches, *a b c*, Fig. 17. 2h 30m the vacancies between the three ſmall projecting ſhallows are now filled up by the ſame cauſe that occaſioned them, ſo as to have given them the ſhape of an uniform but broader ſhallow on the ſide where the points came out.

Particular appearance of the ſhallows. They are tufted like maſſes of denſe clouds.

The ſhallows have none of the appearance called corrugations, but are tufted. The cloſe connection of the tufts makes them appear as if in every vacancy there were clouds under clouds, that prevented our looking far into them. The decay of the ſhallows appears to be produced by the encroachment of the luminous clouds on all ſides, in conſequence of the diminution of the energy which cauſed them."

Of Ridges.

Ridges, or elevations of the luminous clouds of the ſun.

Ridges are elevations above the general ſurface of the luminous clouds of the ſun. The length of one of the longeſt was found to be about 75,000 miles. They generally ſurround the openings, though they are often ſeen in places where there are no openings. They are ſoon diſperſed. From the appearances, the author thinks it probable that the luminous matter is diſturbed at top by the tranſparent elaſtic fluid which iſſues from the openings, or that it may act below the luminous clouds, ſo as to lift them up or increaſe their maſs.

 Of

Of Nodules.

Nodules, or ſmall elevations of the luminous matter.

Nodules are ſmall, but highly elevated luminous places. They may be ridges four ſhortened.

Of Corrugations.

Corrugations are ſmaller elevations and depreſſions of the luminous matter.

The corrugations conſiſt of elevations and depreſſions. They have a mottled appearance, conſiſting of dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places. On favourable days the corrugated ſurface preſents its elevations and depreſſions as diſtinctly as the rough ſurface of the moon. They extend over the whole ſurface of the ſun. Diſperſed ridges or nodules make corrugations. The corrugations change their ſhape and ſituation; they increaſe, diminiſh, divide, and vaniſh quickly. Fig. 18 exhibits a ſmall ſketch of the place of phenomena obſerved by the author and Dr. Wilſon, in which the rapid changes were correſpondently obſerved.

Of Indentations.

Indentations of the luminous matter.

The dark places of corrugations are indentations. That they are not much depreſſed, is deduced from their viſibility pretty near the margin of the ſun. The indentations go down at the ſides like circular arches, but their bottoms are occaſionally flat. See Fig. 19. Some indentations have no openings, others have. The corrugations are of all ſhapes, chiefly lengthened. The indentations are of the ſame nature as ſhallows; are of different ſizes; ſometimes containing very ſmall openings, and ſometimes changing to openings. They are extended over the whole ſurface of the ſun, and with low magnifying powers they appear like points.

Of Pores.

Pores.

The low places of indentation are pores. They occaſionally increaſe, and become openings: and frequently they vaniſh in a ſhort time.

Of the Regions of Solar Clouds.

The ſhining matter of the ſun cannot be a liquid or a gas;

Dr. Herſchel obſerves that the phenomena before deſcribed could not appear, if the ſhining matter of the ſun were a liquid, becauſe by the laws of hydroſtatics the openings, ſhallows,

ſhallows, indentations, and pores would be filled up, and the ridges and the nodules would ſubſide almoſt inſtantly. Whereas many openings have been known to laſt for a whole revolution of the ſun, and extenſive elevations have remained ſupported for ſeveral days. Still leſs could theſe phenomena conſiſt with the ſuppoſition of elaſtic fluidity. It remains, therefore, ſays he, for us to admit this ſhining matter to exiſt in the manner of empyreal luminous or phoſphoric clouds, reſiding in the higher regions of the ſolar atmoſphere. In ſupport of this notion he gives the following obſervations. and conſequently it muſt form clouds.

Changes in the ſolar clouds happen continually. There are two different regions of ſolar clouds, the lower of which conſiſts of clouds leſs bright than thoſe which compoſe the upper ſtratum. The inferior clouds are opaque, the colour of all the ſhallows being the ſame, and conſequently not affected by the cauſe which acts upon the upper ſtratum when ſhallows are generated. By a contrivance for the uſe of his photometer, he found that if the ſuperior ſelf-luminous clouds of the ſun throw the ſame quantity of light on the inferior region of opaque ſolar clouds as they ſend to us, thoſe inferior clouds, of which the ſhallows are compoſed, reflect about 469 rays out of 1000 they receive; but that the ſolid ſurface of the ſun ſeen in the openings reflects no more than about 7. The indentations are planetary clouds, reflecting light through the open parts of the corrugations, and thoſe opaque inferior clouds probably ſuffer but little of the light of the ſelf-luminous ſuperior clouds to ſtrike the body of the ſun. The motion of the inferior clouds is ſeen through the openings as they paſs along. The ſuperior clouds are alſo ſeen to tranverſe the ſame apertures previous to their cloſing. Rapid ſketch of the phenomena of the ſun's ſurface. Luminous clouds. Dark or planetary clouds. Other facts and appearances.

That the planetary clouds are of eminent uſe, is inferred by recurring to the phenomena before recited. The planetary atmoſphere of the ſun, its great height, its denſity, as inferred from the power of gravitation, which is known to be twenty-ſeven times ſtronger at the ſun's ſurface than with us, and muſt accordingly condenſe the atmoſpheric gaſes, its agitation ſimilar to the winds of our planet, and the clear atmoſpheric ſpace beneath the ſhallows, are among the facts upon which our philoſopher extends his diſcuſſions. He repeats and amplifies his remarks reſpecting the operation of the gas, which is ſtated to paſs from the ſun itſelf upwards to the region of Uſe and conſtitution of the ſolar clouds.

the clouds, so as to generate pores, corrugations, and all the solar changes, and most probably to maintain phenomena in the sun's atmosphere, which in ours would be mere transitory corruscations, like those of the aurora borealis, but are there, on account of the greater density, so compressed as to become much more efficacious and permanent. If this account of the solar appearances shall be well founded, he concludes that there will be no difficulty in ascertaining the actual state of the sun with regard to its energy in giving light and heat; and that nothing will now remain but to decide the question which will naturally occur, whether there be actually any considerable difference in the quantity of light and heat emitted from the sun at different times. To ascertain this he has recourse to his journal.

Inquiry whether the energy of the solar heat and light be dependant on these phenomena?

Journal of solar observations.

In the first place he gives a set of observations, in which the signs of scarcity of luminous matter in the sun were apparent; namely, in general a deficiency of the luminous or empyreal clouds, no ridges, nodules, corrugations, or openings. This period lasted from the year 1795 to 1800. A second set, commencing in the year 1800, affords indications of a contrary nature. He expresses his opinion that the character of the seasons may be greatly dependent upon these phenomena. But, in order to ascertain by some correspondent event or consequence that the seasons have been most productive when the solar spots were abundant, he recurs to the astronomy of Lalande for the solar appearances, and to the table in Adam Smith's Wealth of Nations for the prices of wheat during the same periods. The results are, upon the whole, favourable to his theory; though every cautious investigator into the laws of the universe must see that it is greatly in want of the support of observation in both respects. The Doctor, with his usual candour, states some of the difficulties of his subject, and concludes by adding, that his prediction of plentiful harvests from the present thriving state of the solar constitution ought not to be relied on with more confidence, than the facts and arguments he has presented may entitle it to. And from motives of the same kind, I have also taken the liberty to condense this part of his paper rather more than the other.

Opinion that the seasons are governed by the variation in the sun.

Recurrence to Lalande for solar facts, and to Adam Smith for the fertility of the seasons.

IN

IN a subsequent paper of the same volume, Dr. Herschel gives a number of additional observations, made in the spring of 1801, by which it appears that the sun continues to exhibit symptoms of a plentiful emission of light. He strongly suspects that one half of our sun is less luminous than the other, and that its variable lustre may possibly appear to other solar systems as irregular periodical stars are seen by us; but whether this arises from some permanent construction of the solar surface, or be merely an accidental circumstance, must be left to future investigation.

Subsequent observations.

Suspicion that the sun has a bright and an obscure side.

He takes notice that we need not in future be at a loss with regard to the current temperature of this climate, as the thermometrical observations, regularly given in the Philosophical Transactions, afford a proper standard of comparison with the solar phenomena.

Judgment of the seasons by thermometrical registers.

In this paper also, with a drawing, is described a method of substituting liquids instead of dark glasses, for moderating the solar light and heat in telescopes. It is a square trough, closed at two opposite ends by well polished parallel plane glasses. On one side of this trough is a small handle, and on the other a lip or spout for pouring out any portion of the liquid when the rest is to be diluted. The trough is made to fit into an excavation in the eye tube of the telescope, and in this situation the solar rays must pass through the liquid therein contained before they arrive at the eye. By colouring the fluid, the observer has it in his power to soften the light at pleasure; and water keeps off the heat so as to prevent the least inconvenience. It is easy to make trial of a variety of fluids in this way. Among other mixtures, the Doctor found that ink, diluted with water, and filtered through paper, shewed the solar image as white as snow, and very distinct. Through this mixture he could observe the sun in the meridian for any length of time without danger to the eye, or to the glasses, with a mirror of nine inches in diameter, and the eye-pieces open, as they are used for night observations.

Very advantageous use of liquids instead of the dark glasses of telescopes.

Apparatus.

IV.—Ob-

IV.

Obfervations and Experiments upon Dr. James's Powder, with a Method of preparing, in the humid Way, a fimilar Subftance. By RICHARD CHENEVIX, ESQ. F.R.S. M.R.I.A.*

Preliminary remarks.

AFTER the obfervations and experiments made by Dr. Pearfon, to inveftigate the nature of Dr. James's powder, and prefented by him to this Society, very little remained to be effected or defired, towards a further knowledge of the fubject. But thofe very experiments ferved to fuggeft, that the mode of preparation was far from being the beft that the prefent improved ftate of chymical knowledge might afford; and that, in all probability, a lefs defective compofition might refult from a procefs more conformable to fome improvements, which of late have been advantageoufly applied to pharmaceutic chymiftry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or fynthetical, fire (that potent and once believed to be univerfal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and conftant fuccefs. And, if it is ftill recurred to, the advantage of promptnefs, and a remnant of ancient cuftom, are the principal reafons. But, where other methods can be devifed to effect the fame combinations, (and the humid way offers many,) every perfon converfant in chymical knowledge will allow the benefit of adopting them. The recent improvement in the mode of preparing calomel, is a ftriking example of fuch falutary corrections being fuccefsfully introduced.

The method of preparing James's powder, or *Pulvis Antimonialis*.

A few obfervations upon the formula according to which Dr. James's powder, or the *Pulvis Antimonialis*, is prepared, and upon fome properties of antimony, will place this affertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartfhorn fhavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phofphate of lime, which already contains a great excefs of lime, and add to it an oxide of antimony. In

* Philof. Tranf. 1801.

this

this process, it has been supposed, that the phosphoric acid of the bone or hartshorn will saturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little sulphuric acid might, during the process, have been formed by the combustion of the sulphur of the crude antimony, is dissipated, at a much lower temperature than that to which the powder is exposed.

Every oxide of antimony with which we are acquainted, is volatile at a high degree of heat: it would therefore be hazardous to assert, that it is possible to preserve always the same proportion of antimony, whatever care may be employed in directing the operation; and, a dissimilarity in the chemical result, must necessarily be attended with uncertainty in the medical application. It contains an uncertain dose of antimony;

To this property may be added another, no less conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great measure, insoluble in all the acids. What the effect of the gastric juice may be, upon a substance which resists the action even of nitro-muriatic acid, it is not my purpose to determine. It is sufficient for me to say, that, as the quantity of insoluble matter, in a given quantity of Dr. James's powder, prepared at different times, may vary, the effect of any dose also may differ, according to the proportions of soluble and insoluble matter. and the oxide varies in its solubility.

I look upon it as a fortunate circumstance, that those experiments and observations which I mentioned in the beginning of this paper, existed as a standard to which I might refer my own attempts, and by which I might estimate their validity. Dr. Pearson has proved, (as by my own experiments I have found,) that in Dr. James's powder about 28 per cent. resisted the action of every acid. In examining some of the *Pulvis Antimonialis* of the London Pharmacopeia, I found the average quantity of insoluble matter to be about 44 per cent. This proportion, however, was liable to considerable variation.* Facts in proof.

* I find, from the information of several medical gentlemen, that the *Pulvis Antimonialis* is generally considered as stronger than Dr. James's Powder. This seems rather extraordinary, when we consider that the quantity of insoluble matter is greater in the former than in the latter; and would almost lead us to suspect it to be the active part of the medicine.

The

Whether this powder be a ternary compound.

The powder here treated of is denominated, by Dr. Pearſon, a triple ſalt, or a real ternary combination of a double baſis, (lime and antimony,) with phoſphoric acid. What I have mentioned, with regard to the quantity of acid contained in bone or hartſhorn, as being too ſmall to ſaturate a new portion of theſe baſes, may throw ſome doubts upon the poſſibility of any ſuch combination in the preſent caſe. But I have made ſome more direct experiments, which tend to prove, that no ſuch combination does exiſt.

It contains no phoſphate;

I took ſome white oxide of antimony, (formerly called Algaroth Powder,) precipitated by water from muriate of antimony, and heated it for a long time with phoſphoric acid. I decanted the liquor, and waſhed the powder that remained. No antimony could be found in the liquor; nor could any traces of phoſphoric acid be detected in the reſiduary oxide of antimony. I then took a ſolution of muriate of antimony, and divided it into two equal parts: into one, I poured diſtilled water; and, into the other, a ſolution of phoſphate of ſoda. In each liquor, a copious precipitate was formed; which precipitates, after being well waſhed, were dried. The weight of both was the ſame; whereas, it is evident that, had any phoſphoric acid been combined with the oxide, there would have been an augmentation of weight, in that which was precipitated by the ſolution of phoſphate of ſoda. This precipitate likewiſe, upon examination, gave no traces of phoſphoric acid. From theſe experiments it appears, that there exiſts no combination, which can be denominated a phoſphate of antimony.

neither does it appear to be antimoniate of lime.

To attempt an explanation of the real nature of the powder here ſpoken of, I had recourſe to ſome experiments of Monſ. Berthollet. By detonating ſulphuret of antimony and nitrate of potaſh, in a crucible, he obtained a maſs, which he reduced to powder, and waſhed. The liquor gave, upon evaporation, a cryſtallized ſalt, which M. Berthollet terms an *antimoniate of potaſh*. I never could ſucceed in any attempt to form a ſimilar combination between the above white oxide of antimony and potaſh, owing, I believe, to the ſmall quantity of oxigen contained therein, compared with that which is combined with the oxide obtained by detonation. I cannot therefore ſay, that the powder in queſtion is, in any degree, what M. Berthollet would call an *antimoniate of lime*.

But,

But, be the ſtate, whether of mixture or of combination, what it may, my purpoſe is to endeavour to produce a ſubſtance, which, from its more certain mode of preparation, may be more equal and conſtant in its effects.

Diſſolve, together or ſeparately, in the leaſt poſſible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phoſphate of lime. * Pour this ſolution gradually into diſtilled water, previouſly alkalizated by a ſufficient quantity of ammonia. A white and abundant precipitate will take place, which, well waſhed and dried, is the ſubſtitute I propoſe for Dr. James's powder. New proceſs for a combination of antimony of more conſtant effect.

The theory of this precipitation is ſo clear and ſimple, that it does not require any comment. It may be uſeful, however, to thoſe who wiſh to make this preparation, to remark, that it is abſolutely neceſſary that the ſolution of phoſphate of lime and of oxide of antimony, in muriatic acid, ſhould, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, ſhould the alkaline liquor be poured *into the acid ſolution*, the water of the former would act upon the entire maſs of oxide of antimony, while the alkali would precipitate the phoſphate of lime only as it ſaturated the acid which held that ſalt in ſolution: thus, the precipitate would contain more antimony in the beginning; and, towards the end, the phoſphate of lime would be predominant. For the ſame reaſon too, a pure alkali is preferable to its carbonate; for the carbonic acid diſengaged, would retain in ſolution a portion of phoſphate of lime. Remarks.

* In order to procure the phoſphate of lime, I diſſolved in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its ſtate of greateſt cauſticity. By this means, the exceſs of muriatic acid, which held in ſolution the phoſphate of lime, is ſaturated, and the phoſphate is precipitated; but no muriate of lime is decompoſed, if the ammonia is quite free from carbonic acid. This is the moſt direct method of obtaining phoſphate of lime pure. This ſalt is not decompoſed, as ſome have aſſerted, by muriatic acid, but merely diſſolved by it. I have been induced to ſtate fully theſe particulars, becauſe, from the beneficial effects of this ſalt in the treatment of rachitis, as propoſed by M. Bonhomme, (*Annales de Chimie*, Vol. XVIII. p. 113,) it may become of general uſe. The oxide of antimony, I obtained by precipitating, by water, the common butter of antimony of the ſhops.

It is probably a mixture.

Whether this compofition be a chymical combination or a mixture, I will not take upon me to determine; but, for the reafons above mentioned, in fpeaking of Dr. James's powder, I believe it to be merely a very intimate mixture. At all events, it muft be more homogeneous than any that can be prepared in the dry way. It is entirely foluble in every acid that can diffolve either phofphate of lime or oxide of antimony feparately; and, to have it conftantly and uniformly the fame, no further addrefs, in preparing it, is required, than to avoid the errors I have mentioned.

Stronger preparation.

As, after fome medical trials of the powder, it was fuggefted to me, that it might be advantageous to render it fomewhat ftronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phofphate of lime, and precipitating as above defcribed. The medicinal power was then confiderably increafed.

Medical teftimony of its ufe.

Dr. James's powder is a medicine which has been fo long in ufe, and is fo defervedly ranked among the moft valuable we poffefs, that every attempt to render the procefs for preparing it more fimple and more certain, muft be allowed to be of fome importance. But, whatever reafon there was to think, by arguing upon its chymical properties, that I had really fucceeded in improving its medicinal virtues, it ftill remained to be proved, by actual experiment, that the hoped-for fuccefs was not merely conjectural. To afcertain this, I gave fome of my powder to Dr. Crichton, Dr. Babington, and Mr. Abernethy; gentlemen whofe extenfive practice and acknowledged fkill fufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. James's powder and the *Pulvis Antimonialis*; but, that it is more mild, and confequently may be given in larger quantities, feldom producing naufea or vomiting, in dofes of lefs than eight or ten grains.

V.—*Con-*

V.

Conſtruction of an Hydraulic Apparatus, which by Means of the Syphon raiſes Water above its level, and performs its Alternations without Attendance. By Mr. Wm. Close.

To Mr. NICHOLSON.

SIR,

Dalton, Nov. 6, 1801.

At the time when I wrote to you on the application of the ſyphon to raiſe water above the reſervoir, I could find no eaſy method of making the inſtrument afford an equable ſupply without attendance. Since then, however, I have thought of a combination for the purpoſe, applicable to any height under an elevation of thirty feet; and another to raiſe water below the reſervoir, independant of the preſſure of the atmoſphere. New method of raiſing water by the ſyphon.

I ſend you a drawing, and deſcription of each.

Plate III. exhibits a repreſentation of the apparatus for raiſing water above the reſervoir. An air pipe is inſerted into one ſide of the turn of the ſyphon S S, and a ſmall plate is fixed on the ſide of its aperture oppoſite the ſhorter branch to obſtruct part of the current. At ſome diſtance from the ſyphon this pipe divides into two branches, 2, 3, which lead to two veſſels A and B. At the ſeparation is fixed a cock of ſuch a conſtruction, that the air pipe 1 can only communicate with one of its branches at once. The barrel of the cock, or that part which is repreſented by ſhort radial lines in the ſection of this part of the apparatus, has three lateral openings: the plug or key which moves within the barrel contains a duct or channel, which, inſtead of paſſing ſtraight through it, is turned with ſuch a bend, that when one end of it is under the opening of the air pipe, the other will be oppoſite the orifice of one of the pipes 2, 3. The communication from the air pipe to either of its branches is therefore completed by about a quarter of a turn. Drawing and deſcription of apparatus.

The veſſels A and B have each a pipe with a valve at its lower end, connected with the ciſtern C C, to carry up water, and two other pipes, one to let out the raiſed water, and the other to take in air at the ſame time. The air pipe need only be

be very ſmall, but the water pipe muſt be wide enough to empty the veſſel in the ſame ſpace of time that it was filled. Each has a valve, but as they are all upon the ſame level, only the valves of the two water pipes are repreſented. All theſe valves are cloſed by weights. Between theſe veſſels is repreſented a wheel, with a weight ſuſpended on one ſide, and the veſſel E on the other. Its rim has a groove, for the chains or cords, and one ſide is formed into teeth, to act upon another toothed veſſel above, which moves the cock in its axis.

The veſſel D receives water from the ciſtern C C, through an aperture of a proper width, and is emptied by a ſyphon as often as the water riſes to a certain height.

The ſyphon S S is about ſix inches in diameter; its ſhorter branch riſes thirty feet above the reſervoir, and its longer branch reaches five or ſix feet below the ſurface of the water.

Its effects.

Suppoſe now that this inſtrument is ſet to work and ſupplied with a ſufficient quantity of water. The cock in the air pipe 1 above the veſſel A muſt be opened, and the vertical pipes A C, and B C, muſt be filled with water, by opening a very narrow paſſage between the air pipe and each of its branches, while the valves of the veſſels are cloſed.

This mode of proceeding is adviſeable to prevent the ſudden preſſure of the atmoſphere from forcing the air out of the empty pipes, with ſuch rapidity as would deſtroy the operation of the ſyphon. After this, if all the valves are in good order, and all parts of the apparatus complete, no further attention will be neceſſary.

The periphery of the lower wheel moves between the valves of the veſſel A on one ſide, and thoſe of B on the other, and has a claw or pin to depreſs each in due order. The weight ſuſpended upon the lower wheel opens the valves of B, and by the aſſiſtance of both wheels opens a communication between the air pipe 1, and the veſſel A, whoſe valves have previouſly cloſed their pipes. The preſſure of the atmoſphere accordingly fills A with water, and the air it contained is carried down the ſtream through the longer branch of the ſyphon. About this time the veſſel D is filled to a level with the bend of its ſyphon which conveys water in the veſſel E. This laſt veſſel being filled, overbalances the weight, and moves the two wheels; the valves of the veſſel B are cloſed by their reſpective weights, and the rarefying tube is made to

to communicate with the branch 3; the aperture of the key duct, which before correfponded with the end of the branch 2, is now placed under the orifice of the rarefying tube, and the opening which was there before, is turned to the branch 3.

At the time when all communication between the veffel and fyphon is interfected, one of the claws or pins on the farther fide of the lower wheel, opens the valve of the air pipe in the veffel A, and immediately after it the valve of the pipe for letting out water is depreffed by a claw on the nearer fide. This laft valve will make little refiftance, as the whole preffure of the atmofphere was removed by opening the fmall pipe firft; for this purpofe it has a feparate valve.

The raifed water flows out of A while B is filling.

When the veffels D and E are emptieft, the large weight moves the wheels and the cock; the valves of B are opened in the fame order as thofe of A, which being again connected with the fyphon, is filled while B is emptied.

Remarks and obfervations.

In actual work the receptacle of raifed water muft be placed under the veffels, the axis of the lower wheel may extend acrofs the receptacle, and have two arms or levers, fixed in a proper pofition to deprefs the valves, while the wheel itfelf will move on the outfide. The key or plug of the cock turned by the axis of the higher wheel, fhould move very true in its barrel. The higher wheel fhould be made light if the key of the cock is fixed faft to its axis. The key or plug fhould be frequently taken out and rubbed with tallow; its tightnefs may be adjufted by a fcrew fixed in a proper frame and moved by a weight. When any repair is requifite, the cock in the air pipe above A muft be turned.

A fyphon fix inches in diameter will require its air pipe to be about one inch and a half in diameter.

By a fyphon of thefe dimenfions water may be raifed to the height of fixty feet, if the vertical water pipe of the veffel B, be placed in the ciftern which receives the raifed water from A, and the branch of the air pipe 3 be carried to the top of the veffel B. Both the veffel and the air pipe fhould be made fomething lefs on account of the greater body of air to be rarefied. The air pipe fhould reach confiderably above the veffel, or it muft have a float valve, to prevent any wafte of the raifed water.

If

If the lower end of the pipe B C was inserted into the lower side of the vessel A, the water might be forced out of this vessel into the higher, by admitting the pressure of the atmosphere. The higher water pipe however, should descend considerably below the bottom of the vessel, and its valve should be always kept in good order, or part of the raised water will be drawn down again when the air in the lower vessel is rarefied. Indeed if we can insure the tightness of the valves, water may be carried through any number of vessels, without being delivered into any cistern before the requisite height is obtained.

Construction of the apparatus for raising water below the reservoir.

The apparatus represented in Pl. III. may be used to raise water below the reservoir, if the vessels and their water pipes are sufficiently settled, and the air pipe 1 placed in a vertical position; but for this purpose the combination represented in Pl. IV. will be preferable. It is an inversion of the former method. A stream of water falls down the pipe L, passes through the vessel V, and ascends again through the shorter pipe S. When there is a sufficiency of water to fill the longer pipe, the stream flows through the apparatus with considerable celerity. The longer pipe L having several small holes at the turn near the higher end, a quantity of air is drawn into the stream, and descends into the vessel V, where it quits the current, rises to the higher part of the vessel, and depresses an equal bulk of water, which is carried through the pipe S. This explains the principle.

The air however is not suffered to collect in the vessel V, but is conveyed through the pipe 1, into one of the vessels A B, previously filled with water, which it forces into the stream. The alternate connection of 1, 2, and 1, 3, is completed by a cock of the same construction, as is described in the explanation of Pl. III. and by the same kind of machinery.

The lower parts of the apparatus, Pl. IV. to above the vessels A B, are immersed in water. At present the vessel V communicates with the vessel A; the water will accordingly descend out of A through the tube below 2, which has a valve opening downwards. A small conical valve at the lower end of a pipe inserted into the top of B, being opened immediately before the communication between V and A is completed, the condensed air rushes out of B, and water enters through an aperture under that letter, while the valve under 3 is closed.

 When

When the veſſel B is filled, and A is emptied, the cock in the air pipe begins to be put in motion, but before the tube 1 is connected to the branch above 3, the ſmall conical valve above B cloſes its air pipe; the valve under B being now cloſed by the preſſure of the water in the tubes L S, the water in B is forced down the pipe under 3, and carried away with the ſtream: the condenſed air is let out of A, and this veſſel replenished with water.

The air which is carried down the ſtream while it can enter into neither veſſel above, will collect in V, until an avenue to one is completed, through which it will rapidly aſcend. If the engineer does not like the conſtruction of the cock I have recommended, its place may be ſupplied by two of the common kind, with adjuſting pieces upon their keys.

The condenſed air collected in the veſſel V, may be applied to the purpoſe of raiſing water either above the reſervoir C C, or below the veſſel V, merely by forcing the air out of V into the veſſel containing the water, by the preſſure of the water in the pipes L S.

The apparatus will begin its operations without any trouble; the capacities of the veſſels A B are indeterminate; and if the higher orifice of the pipe S be contracted, the machine will work with a leſs quantity of water than it may be calculated to receive. The veſſels A and B however, ſhould always be filled completely with water before the admiſſion of the condenſed air, otherwiſe the quantity of raiſed water will decreaſe.

Experimental trial.

A model was conſtructed to illuſtrate the principle: the two water pipes were about half an inch in diameter, and only one ſmall hole was made at the higher end of the longer. Into the top of the veſſel repreſented by V, was ſoldered two ſmall pipes, which were ſurrounded by a little thread, and inſerted into the neck of a ſmall glaſs bottle. The veſſel V and the lower parts of the water pipes were plunged into a veſſel of water, until all the air in the apparatus below the ſurface was expelled. In this ſituation the bottle, previouſly filled with water, was fixed upon the pipes above V. The model being lifted out of the water, and the longer pipe fixed to a ſmall ciſtern, the remaining parts of the water pipes were carefully filled; a ſufficient ſupply was let into the ciſtern, and the higher

higher orifice of S was opened. When the stream had acquired its full force, bubbles of air entered the bottle, and the water was quickly expelled.

I am, SIR,

Your humble Servant,

WILLIAM CLOSE.

ERRATA in the Paper on Wind Instruments. (Philos. J. V. 213.)

Page 217 *line* 23, erase *major*.
—— 219 — 44, for *fifth* read fourth.
—— 221 — 11, change G, a, B, into G, A, B.

VI.

Outline of the Properties and Habitudes of the Metallic Substance, lately discovered by CHARLES HATCHETT, *Esq. and by him denominated Columbium.*

ON the 26th of November last a Paper was read at the Royal Society, announcing the discovery and investigation of the properties of what appears to be a new metal; and as by the course of publication it must be several months before it will appear in the Transactions, I shall present my readers with the following outline:

History of the mineral.

1. The mineral was sent with some iron ores to Sir Hans Sloane by Mr. Winthrop of Massachusets, and there is therefore every reason to believe, that it came from some of the iron mines in that province.

External character.

2. It is heavy, and of a dark grey nearly black; it in some measure has the appearance of the Siberian chromate of iron.

Habitudes with acids.

3. The nitric, muriatic, and sulphuric acids act but very feebly upon the mineral; of these however, sulphuric acid produces the greatest effect, and dissolves some iron.

With potash by fusion, and subsequent digestion in muriatic acid.

4. When melted with five or six parts of carbonate of potash it is partially decomposed; but in order to effect a complete decomposition, the ore must be alternately melted with potash, and digested with muriatic acid, which takes up the iron.

5. During

5. During the fufion, carbonic acid is expelled, and the pot afh becomes partially neutralized by a metallic acid, which may be feparated after folution in water, by nitric acid added to excefs, and then appears in the form of a copious white flocculent precipitate. The potafh holds a metallic acid; precipitable by nitric acid.

6. The ore confifts of more than three fourths of this fubftance combined with iron. This acid very abundant

7. The white precipitate is infoluble in boiling nitric acid, and remains perfectly white. Infoluble in nitric;

8. Boiling muriatic acid diffolves it when recently feparated from pot afh. But foluble in muriatic;

9. Sulphuric acid when ftrongly heated alfo diffolves it. And fulphuric acid.

10. The acid folutions when faturated by alkalis afford white flocculent precipitates; pruffiate of pot afh forms an olive green ditto; tincture of galls, deep orange coloured ditto; water alfo when copioufly added to the fulphuric folution precipitates this fubftance in the ftate of fulphate, which as it becomes dry, from white, changes to blue, and laftly to grey. Precipitable.

11. Zinc forms a white precipitate.

12. The white precipitate combines with pot afh and foda, both by the dry and humid ways; it expels carbonic acid, and with pot afh forms a glittering fcaly falt, which has much the appearance of the boracic acid. Neutral falts and their affections.

13. Acids feparate it from the fixed alkalis, and when added to excefs do not diffolve it unlefs heated, but even then nitric acid has no effect.

14. The fame is obferved when alkalis are added in excefs to the acid folutions.

15. Hydro-fulphuret of ammoniac added to the alkaline folutions forms a chocolate coloured precipitate.

16. Ammoniac will not combine with the white precipitate.

17. When pruffiate of pot-afh, or tincture of galls, are added to the alkaline folutions, no effect is produced till an acid is added, and then the ufual olive green and orange coloured precipitates are obtained.

18. The acid and alkaline folutions are colourlefs.

19. The white precipitate will not combine with fulphur in the dry way.

20. It forms a purplish blue glafs with phofphate of ammoniac.

21. It reddens litmus paper.

22. It appears to be of extreme difficult reduction.

Conclufion.

From the above properties it appears to be an acidifiable metal different from thofe already known, and it has been therefore diftinguifhed by the name of Columbium.

VII.

On the Sebacic Acid, or Acid of Fat. By CIT. THENARD *.

Authors who have treated the fubject.

A GREAT number of chemifts have fucceffively written on the febacic acid. Grutmacher appears to have been the firft who has fpoken of it; it has fince been treated of by Rhades, Knap, Segner, and Haller, who have confirmed and augmented his experiments. Crell has given us feveral proceffes for obtaining this acid in a ftate of purity, and has fubjected it to a number of experiments, the refults of which Citizen Guyton has inferted in the *Journal de Phyfique*, (Vol. XVIII. pages 110 and 383, and Vol. XIX. page 384.) Bergman has afcertained the order of the attraction of this acid for the falifiable bafes. Citizen Guyton has handled the fame fubject, and difcovered feveral errors which had efcaped the notice of this chemift.

Object of proof by them differs confiderably from the refult of the prefent experiments.

The object of all the experiments which I have juft mentioned, is to prove that the product of the diftillation of fat contains a peculiar acid, of extreme volatility, and of fuch a poignant and fuffocating nature, that it cannot be refpired without fome degree of danger. Thofe of which I am about to give an account prove 1. That this product actually contains a peculiar acid, which however, fo far from being volatile, odorous and fuffocating, is on the contrary folid and inodorous; 2. That it alfo contains acetous acid; 3. That this new acid has no part in the fmell of diftilled fat; 4. That by all the proceffes that have hitherto been employed for extracting the febacic acid, nothing is obtained but a foreign acid, and that confequently the febacic acid has not yet been known.

* Communicated to the National Inftitute of France, and inferted in the Annales de Chimie, XXXIX. 193.

A. Processes for obtaining the Sebacic Acid, and Characteristic Properties of this Acid.

HAVING distilled a considerable quantity of the fat of pork, I treated the product, at several times, with hot water, and poured acetite of lead into the liquor; a flaky precipitate was formed, which I collected and dried, after which I put it, together with sulphuric acid, into a retort and heated it. The liquid of the receiver had no character of acidity; but in the retort a melted substance floated at the top, analogous to fat, which I carefully separated, and after having washed it well, boiled it with water. By the aid of the action of the heat, the water totally dissolved it, and by refrigeration crystalline needles were deposited, having little consistence: these needles were acid, and possessed properties altogether peculiar to themselves. In order to assure myself that they were not the product of the sulphuric acid, I treated distilled fat with water; and having filtrated and evaporated the liquor, I obtained needles possessing precisely the same properties.

Pork fat was distilled, washed with hot water, and acetite of lead poured in. The precipitate was dried and then distilled with sulphuric acid. No acid came over; but a matter resembling fat, floated in the retort. This being boiled with water proved soluble and crystallized by cooling. It is a peculiar acid; and may be obtained by washing distilled fat with water, without the use of sulphuric acid.

In order to procure this acid, we may also, if we think fit, after having treated the distilled fat with water, saturate the filtrated liquor with pot ash, evaporate it, and pour into it a solution of lead. A precipitate is formed, which is a sebate of lead, that is to be treated as above with the sulphuric acid. These are the three methods which may be employed in order to obtain the sebacic acid: its properties are the following:

On the aqueous solution from distilled fat may be saturated with pot ash, then evaporated and decomposed by acetite of lead. The sebate of lead may then be made to yield its acid by the stronger attraction of the sulphuric.

It is without smell; its taste is slightly acid: it melts like a kind of fat; it considerably reddens the tincture of Tournesol; it is much more soluble in a hot than in a cold temperature. Boiling water, saturated with the sebacic acid, becomes solid by refrigeration; alcohol dissolves a large quantity of it; it crystallizes in small needles; by using proper precautions, we may obtain it in the form of long, large, and beautiful lamellæ, of a very brilliant appearance; it precipitates the acetite and the nitrate of lead, the nitrate of silver, the acetite, and the nitrate of mercury; it saturates the causticity of the alkalies, with which it forms soluble salts; with pot ash it produces a salt which does not attract moisture from the atmosphere, which has but little taste, and which, if we pour upon it sulphuric, nitric, or muriatic acid, becomes turbid, and deposits sebacic

Sebacic acid has no smell; is slightly sour; melts like fat; reddens turnsol; is more soluble in hot than cold water, and so much in the latter as to become solid by cooling. Alcohol plenti-

fully diffolves it. Cryftallizable. Its peculiar attractions and habitudes.

febacic acid; when its folution is concentrated and mixed with one of thefe acids, it becomes folid; finally it does not render turbid the waters of lime, barytes and ftrontites. Thefe properties diftinguifh it from all the other acids, and evidently prove that it is an acid *fui generis*.

B. *Means of feparating the Acetous Acid from the Product of the diftillation of Fat.*

The aqueous folution from diftilled fat contains acetous or febacic acid, more largely the greater or lefs the heat of diftillation. The former is feparated by neutralizing with potafh, and diftilling with fulphuric acid.

THE product of the diftilled fat is treated with water; the liquor is faturated with potafh, and evaporated. When the fubftance is dry, it is introduced into a retort with dilute fulphuric acid, or phofphoric acid, and diftilled: an acid is obtained, which has all the characters of acetous acid; with potafh it forms a foliated falt. This falt is fufceptible of being melted by means of heat; when expofed to the air, it deliquefces fpeedily and completely; its tafte is extremely pungent; with a folution of nitrate of mercury, it forms a precipitate cryftallized in the form of fpangles. When fulphuric acid is poured upon it, acetous acid is difengaged in abundance. Sometimes the water with which the product of the diftilled fat has been treated, contains hardly any thing elfe than acetous acid, fo that in order to obtain acetite of potafh, nothing more is neceffary than to evaporate the liquid. The quantity of febacic and of acetous acid formed in the diftillation of fat, varies in proportion to the leffer or greater degree of heat.

C. *Examination of the Odorous Matter, and of the diftilled Fat.*

The pungent odour of diftilled fat is not produced by an acid.

I INTRODUCED fat, which had juft been diftilled, and the fmell of which was extremely pungent, into a tubulated retort. I adapted to the neck of the retort, a receiver which contained tincture of Tournefol. Having diftilled with a gentle heat, I thereby filled the receiver with a ftrong odour, and yet the colour of the tincture was not changed—a convincing proof that the fmell of diftilled fat is not owing to an acid; befides, if this fmell depended upon an acid, it would probably difappear upon placing it in contact with the alkalies, as the acid would then be abforbed; but this does not happen. We muft therefore conclude, that it depends upon a portion of the fat, converted into gas, and undoubtedly changed in its nature.

D. *Examination of the Proceſſes that have hitherto been employed for obtaining Sebacic Acid.*

CRELL, in order to ſeparate the ſebacic acid from the product of diſtilled fat, firſt added to it a certain quantity of potaſh, after which he filtrated and evaporated it. As the ſalt which he obtained was mixed with oil, he calcined it, after which he re-diſſolved it in water, and evaporated the ſolution. By this means he obtained a ſalt of conſiderable whiteneſs, and of a foliated texture; this he introduced into a retort, with ſulphuric acid, and diſtilled it, whereby he obtained a pungent and fuming acid. But as this proceſs appeared to him to be an inconvenient one, he had recourſe to the following:

Crell's proceſs by lixiviation of fat with potaſh, filtration, evaporation, calcination, and re-ſolution of the ſalt; and laſtly diſtillation with ſulphuric acid.

Perſuaded that the acid of fat exiſted ready formed in the fat itſelf, and was not a product of diſtillation, he prepared a ſoap with fat and potaſh, and treated this ſoap with water, in order to diſſolve the ſebate of potaſh formed in it. But as the water, beſides the ſebate of potaſh, diſſolved alſo a certain quantity of fat combined with potaſh, he added to the ſolution of this ſebate of potaſh and of this ſoap, a ſufficient quantity of alum. By this means he obtained ſebate of potaſh, which had only an admixture of ſulphate of potaſh; this he evaporated, poured ſulphuric acid upon the dry ſubſtance, applied heat; and ſebacic acid paſſed into the receiver.

Another by forming ſoap and treating with water, and ſome alum and diſtilling as before.

In the chemiſtry of Dijon, we find a proceſs different from that of Crell. According to this proceſs, the fat is calcined with a certain quantity of lime, in a crucible; the ſubſtance is then lixiviated with a large quantity of water; the water which holds the calcareous ſebate in ſolution, is evaporated; this calcareous ſebate is introduced into a retort with ſulphuric acid, and the ſebacic acid paſſes into the receiver.

Dijon proceſs. Calcination of fat with lime, lixiviation, evaporation, and diſtillation with ſulphuric acid.

Theſe three proceſſes I have repeated with the utmoſt care, and have obtained the following reſults. The firſt of Crell's proceſſes, and that of the chemiſtry of Dijon, afforded me an acid which has all the characteriſtics of the acetous acid: with potaſh it forms a foliated ſalt, which is deliqueſcent, has an extremely pungent taſte, and on being treated with ſulphuric acid, yields a large quantity of vinegar. If Crell, as he aſſerts, has obtained a fuming and pungent acid, I preſume that it is a ſmall quantity of ſulphureous acid, proceeding from the decompoſition

Crell's firſt proceſs afforded acetous acid.

decompofition of a certain portion of fulphuric acid by the fat, or of the carbone of the acetous acid difengaged from its combination.

His fecond by mifmanagement gave muriatic acid.

In following the fecond procefs defcribed by Crell, we do not obtain any acetous acid, but an acid, which is nothing elfe but the muriatic. In fact, it forms with the nitrate of filver a precipitate infoluble in an excefs of nitric acid; with foda it yields cubic cryftals. If we pour fulphuric acid upon thefe cryftals, a penetrating gas is difengaged, which, on being brought into contact with the air, gives rife to vapours: the fame acid, mixed with nitric acid, diffolves gold. With the oxide of mercury it forms a volatile falt; with potafh it forms a falt capable of being fufed without undergoing decompofition. Thefe circumftances render it probable that Crell has employed the potafh of commerce, which always contains muriate of potafh; for in repeating this procefs with pure potafh, no acid is obtained, except an almoft imperceptible quantity of vinegar. This vinegar is formed by treating the fat with potafh and the fulphuric acid; for fat contains no acid, not even when it is rancid. At leaft, I have feveral times treated rancid fat with water, and have uniformly obtained a liquid, which did not redden the tincture of Tournefol.

But if well performed, vinegar.

E. *Recapitulation.*

Enumeration and comparifon of the above refults.

THESE experiments prove, in my opinion, what I have advanced in the beginning of this memoir; namely, that there exifts in the product of diftilled fat a peculiar acid, which, inftead of being volatile, odorous, fuffocating, is on the contrary folid, inodorous, and fixed to a certain degree; that, befides this acid, the product of diftilled fat contains acetous acid; that the febacic acid has no fhare in producing the fmell of diftilled fat, which probably depends upon fome particles of fat, converted into vapour and altered in their nature. They prove, befides, that by the proceffes of Crell, and by that defcribed in the chemiftry of Dijon, we obtain only the muriatic or the acetous acid; that confequently the febacic acid has hitherto remained unknown, and that in the prefent ftate of our knowledge it is a new acid.

Hog's lard only was ufed.

I ought to mention, that all the experiments which I have related have been made with hog's lard, and that I have not, like Crell, varied thefe experiments with human fat, the marrow

marrow of beef and tallow. I propose to repeat them with these different fatty substances, and think, even according to the experiments of Crell, that I shall obtain the same results. I have not as yet examined all the properties of the sebacic acid; but it is my intention to study them with care, and give an account of them to the class.

VIII.

Improvement in the Art of preparing Radical Vinegar. By CIT. Y. PERES, *Jun.**

THE experiment which enabled me to ascertain the real difference between the acetous and the acetic acid, and afterwards to propose to the learned a new theory of vegetable acidification in general, may be of great service in the art of preparing what is termed radical vinegar; or, to speak more accurately, it ought totally to change the process used for preparing this substance.

Observations on the distillation of acid from acetite of copper.

The process at present in use, consists in distilling the acetite of copper in large spherical vessels of stone ware. By taking the greatest care, a small portion is obtained of this *radical vinegar*, so much used in medicine and for domestic purposes. The difficulty of checking the expansion of this product, is an inconvenience which, being dependent on the nature of the process itself, has at all times been felt. Some improvements have been proposed; but as the nature of the operation was not understood, these improvements produced scarcely any advantage, and the process still remains very defective. It is easy to observe, that the violent heat which is used, is much more than sufficient for depriving the acetous acid of its carbon, and that it must reduce a portion of it to its first elements. I do not hesitate to assert, that at least half the radical vinegar is lost: The large quantity of gas that is obtained, incontrovertibly proves the truth of this assertion.

Radical vinegar is deduced to be acetous acid deprived of carbon.

I have formerly shewn, by various deductions from earlier experiments, that *radical vinegar* is nothing but acetous acid deprived of carbon. I have shewn, that the ordinary process

* In a letter to the Editor of the Mag. Encyclop.

for

for preparing this acid effects nothing more than to abftract this excefs of carbon; and I have completed my proofs, by preparing this acid myfelf, by a means which every one knows can have no other effect than to abftract carbon from the fubftances to which it is applied.

Simple procefs for making it. One part of fulphuric and two of vinegar are diftilled.

I diftilled a kilogramme of fulphuric acid with two kilogrammes of good white vinegar. I fuddenly brought the mixture to ebullition, and obtained a very large quantity of radical vinegar, as white and pungent as that of commerce. This procefs is fo fimple and economical, that I thought it would be ufeful to extract it from the Memoirs which I have given upon this branch of chemiftry, and offer it to manufacturers. I can affure them, that it will diminifh the expences of the manufacture by three fourths. In fact, the fulphuric acid which remains may ftill ferve for two more operations: but then it will be neceffary to rectify the radical vinegar, for it will be found impregnated with fulphurous acid gas. It would be proper to try whether the action of manganefe, which is ufed for ether, might not be applied to this rectification. I do not apprehend that this metal, in fo high a degree of oxidation, is fufceptible of being attacked by the acetic acid.

Manganefe propofed.

Erroneous notion of the elective attractions of acetic acid.

An erroneous opinion obtains, with refpect to the tendency of this acid to combination, which I think it incumbent upon me to refute. Chemifts place it, in their tables of attractions, in a much higher rank than it will be found entitled to, upon an inveftigation of its properties with the leaft degree of accuracy. We find that it difplaces only the carbonic, acetous and other weak acids. This error has arifen from the appearance of ftrength which it derives from the pungency of its fmell. In this inftance, however, the chemical properties of the fubftance are by no means proportionate to the impreffion it makes upon our fenfes. I fhall add an obfervation which proves that this acid is much lefs powerful than is generally imagined; namely, that the vapour which it fpontaneoufly emits, and which might feem to be its moft acid portion, fcarcely reddens paper tinged with tincture of tournefol. In fact it is nothing more than a modification of hidrogen, and it takes fire like ether.

Its fumes are fcarcely acid, &c.

IX.

An Account of a new Eudiometer. By Mr.* H. DAVY, *Director of the Chemical Laboratory, and Lecturer on Chemistry to the Royal Institution.*

THE dependance of the health and existence of animals upon a peculiar state of the atmosphere, and the relations of this state to processes connected with the most essential wants of life, have given interest and importance to inquiries concerning the composition and properties of atmospheric air.

Composition of the atmosphere an interesting subject.

This elastic fluid has been long known to consist chiefly of oxigen and nitrogen, mingled together, or in a state of loose combination, and holding in solution water.

Oxigen and nitrogen.

A variety of processes have been instituted with the view of determining the relative proportions of the two gases, but most of them have involved sources of inaccuracy; and lately all, except two (the slow combustion of phosphorus, and the action of liquid sulphurets), have been generally abandoned.

The eudiometric processes commonly used are exceptionable.

Both phosphorus and solution of sulphuret of potash absorb the whole of the oxigen of atmospheric air at common temperatures, and they do not materially alter the volume, or the properties of the residual nitrogen; but their operation is extremely slow; and in many cases it is difficult to ascertain the period at which the experiment is completed.

I have lately employed as an eudiometrical substance the solution of green muriate, or sulphate, of iron, impregnated with nitrous gas; and I have found that it is in some respects superior to many of the bodies heretofore used, as it rapidly condenses oxigen without acting upon nitrogen; and requires for its application only a very simple and a very portable apparatus.

Solution of muriate, or sulphate, of iron, impregnated with nitrous gas, proposed to absorb oxigen.

This fluid is made by transmitting nitrous gas through green muriate, or sulphate, of iron, dissolved to saturation in water†. As the gas is absorbed, the solution becomes of a deep olive brown, and when the impregnation is completed it appears

How made.

* Journals of the Royal Inst. p. 45.

† Dr. Priestley first observed this process: for a particular account of it, see Researches, Chemical and Philosophical, p. 152. Johnson.

opaque

opaque and almoſt black. The proceſs is apparently owing to a ſimple elective attraction; in no caſe is the gas decompoſed; and under the exhauſted receiver it aſſumes its elaſtic form, leaving the fluid with which it was combined unaltered in its properties.

Eudiometer tube.

The inſtruments neceſſary for aſcertaining the compoſition of the atmoſphere, by means of impregnated ſolutions, conſiſt ſimply of a ſmall graduated tube, having its capacity divided into one hundred parts, and greateſt at the open end; and of a veſſel for containing the fluid.

Manipulation.

The tube, after being filled with the air to be examined, is introduced into the ſolution; and, that the action may be more rapid, gently moved from a perpendicular towards a horizontal poſition. Under theſe circumſtances the air is rapidly diminiſhed; and, in conſequence of the dark colour of the fluid, it is eaſy to diſcover the quantity of abſorption. In a few minutes the experiment is completed, and the whole of the oxigen condenſed by the nitrous gas in the ſolution in the form of nitrous acid.

Maximum of diminution.

In all eudiometrical proceſſes with impregnated ſolutions, the period at which the diminution is at a ſtand muſt be accurately obſerved; for, ſhortly after this period, the volume of the reſidual gas begins to be a little increaſed, and, after ſome hours, it will often fill a ſpace greater by ſeveral of the hundred parts on the ſcale of the tube, than that which it occupied at the maximum of abſorption.

Subſequent increaſe; and why.

This circumſtance depends upon the ſlow decompoſition of the nitrous acid (formed during the experiment), by the green oxide of iron, and the conſequent production of a ſmall quantity of aeriform fluid (chiefly nitrous gas) *; which, having no affinity for the red muriate, or ſulphate, of iron produced, is gradually evolved, and mingled with the reſidual nitrogen.

The muriate is preferable to the ſulphate.

The impregnated ſolution with green muriate is more rapid in its operation than the ſolution with green ſulphate. In caſes

* The decompoſition of nitrous acid, by ſolutions containing oxide of iron, as its minimum of oxidation, is a very complex proceſs. The green oxide, during its converſion into red oxide, not only decompoſes the acid, but likewiſe acts upon the water of the ſolution; and ammoniac is ſometimes formed, and ſmall portions of nitrous oxide and nitrogen evolved with the nitrous gas.

when

when these salts cannot be obtained in a state of absolute purity, the common or mixed sulphate of iron may be employed. One cubic inch of moderately strong impregnated solution is capable of absorbing five or six cubic inches of oxigen, in common processes; but the same quantity must never be employed for more than one experiment.

Effect.

A number of comparative experiments, made on the constitution of the atmosphere at the Hotwells, Bristol, in July, August, and September, 1800, with phosphorus, sulphurets of alkalis, and impregnated solution, demonstrated the accuracy of the processes in which the last substance was properly employed. The diminutions given by the sulphurets were indeed always greater by a minute quantity than those produced by phosphorus and impregnated solutions: but the reason of this will be obvious to those who have studied the subject of eudiometry. In no instance was it found 100 parts in volume of air contained more than 21 of oxigen: and the variations connected with different winds, and different states of temperature, moisture, &c. were too small, and too often related to accidental circumstances, to be accurately noticed.

Comparative experiments, or trials.

In analysing the atmosphere in different places, by means of impregnated solutions, I have never been able to ascertain any notable difference in the proportions of its constituent parts. Air, collected on the sea at the mouth of the Severn, on October the 3d, 1800, which must have passed over much of the Atlantic, as the wind was blowing strong from the west, was found to contain 21 per cent. of oxigen in volume; and this was nearly the proportion in air sent from the coast of Guinea, to Dr. Beddoes, by two surgeons of Liverpool.

No remarkable difference observable in the air of various places as to its oxigen;

If we compare these results, with the results gained more than twenty years ago, by Mr. Cavendish, from experiments on the composition of atmospherical air, made at London and Kensington; considering, at the same time, the researches of Berthollet in Egypt and at Paris, and those of Marti in Spain, we shall find strong reasons for concluding, that the atmosphere, in all places exposed to the influence of the winds, contains very nearly the same proportions of oxigen and nitrogen: a circumstance of great importance; for, by teaching us that the different degrees of salubrity of air do not depend upon differences in the quantities of its principal constituent parts, it ought to induce us to institute researches concerning the different

—and consequently its relative salubrity depends on substances dissolved or suspended in it.

ferent fubftances capable of being diffolved or fufpended in air, which are noxious to the human conftitution: particularly as an accurate knowledge of their nature and properties would probably enable us, in a great meafure, to guard againft, or deftroy, their baneful effects.

X.

Analyfis of a newly difcovered Mineral refembling the Hyacinth. By Profeffor TROMSDORFF.*

History.

THROUGH the kindnefs of Prince Gallitzin, I received a new mineral fubftance, denominated by him *red garnet* (rather granat), which greatly refembled the hyacinth in refpect of colour, hardnefs, fpecific gravity, &c. The native country of this new foffil is Greenland. I fhall call it *compact hyacinth* (dichter hyacinth.)

Action of heat.

In order to inveftigate the nature of this foffil, it was ignited intenfely for two hours, and then fuffered to cool. It fuffered no lofs of weight, nor was its beautiful red colour in the leaft impaired. It was fubmitted to a fimilar action of caloric fix different times, but no change was effected. It was then reduced to a powder.

Powder boiled with potafh, and then fufed.

A. Two hundred grains of the impalpable powder were boiled for fome time in a filver crucible, with a folution of potafh, containing 500 grains of dry alkali. The fluid, after having been boiled for fome time upon the foffil, acquired a yellow colour, and at laft became green. It was evaporated to drynefs, re-diffolved in diftilled water, evaporated again, and laftly ignited at as high a temperature as the filver crucible would endure.

Mafs, difficultly foluble in water, depofits a red powder.

B. The fufed mafs of the foregoing procefs, when cold, was of a dark green colour, inclining to a paler green on the fides which had been in contact with the crucible. Water feemed to have little action upon it, but after having been boiled in this fluid for a confiderable time, it began to foften, and was at laft diffolved. The clear folution obtained in this manner, after having been fuffered to ftand undifturbed, depofited a

* Tranflated from Crell's Chem. Annalen, 1801, Part VI. page 1.

reddifh

reddish brown powder, greatly resembling the red sulphurated oxide of antimony. The supernatant fluid was perfectly transparent.

C. The whole fluid was then warmed a little, and muriatic acid added to it gradually. The powder before separated became thus soluble after this acid had been added in excess. It now acquired a dark green colour, and was perfectly transparent.

Addition of muriatic acid completes the solution.

D. Having concentrated this fluid down to two ounces, no siliceous earth being deposited, it was evaporated to perfect dryness. The residue obtained was of a white colour. After being ignited it weighed 110 grains. It was boiled again with pure potash in a silver crucible, evaporated to dryness, and fused. The fused mass was again dissolved in boiling water, muriatic acid was added in excess, and the whole evaporated to dryness. On adding some distilled water to the residue, an insoluble powder was obtained. This powder, after being washed in boiling distilled water, was dried, and then ignited. Its weight amounted to 100 grains. *It was pure siliceous earth.*

Evaporation to dryness, fusion with potash, solution and evaporation, as before, and the addition of water, separated much *silex.*

E. The fluid from which the foregoing product had been obtained, together with all the water expended in washing it, was collected together, evaporated, and mixed, whilst *boiling hot*, with a solution of muriate of potash. A reddish brown precipitate fell down, which, being collected upon a filtre, was washed in distilled water.

Precipitate, from last solution, by muriate of potash;

F. The precipitate, after having acquired a considerable degree of dryness on the filtre, was boiled during four hours in a concentrated solution of pure potash. The fluid was then attenuated with a considerable quantity of boiling distilled water, and transferred upon a filtre, of which the weight had been previously ascertained. The residue obtained, after a repeated ebulition with boiling distilled water, was suffered to dry, and then strongly ignited. Its weight was 32 grains. It was marked X.

was dried, boiled in potash, then diluted, separated, and ignited.

G. The aqueous solution of potash, together with all the water used for washing the last obtained products, was concentrated by evaporation. On adding muriatic acid to it, a precipitate ensued; an excess of acid was therefore added, in order to re-dissolve it. The solution was then decomposed by means of carbonate of ammmonia. The precipitate now obtained

The last solution was super-saturated with muriatic acid, and precipitated by carbonate of ammonia. It gave *alumine.*

tained was of a white colour. After being collected on a filtre, washed, and dried, its weight amounted to 56 grains. On dissolving it in sulphuric acid, and adding a minute quantity of potash, pure acidulous sulphate of alumine and potash was obtained. It was therefore pure *alumine*.

The precipitate X, ignited with potash and washed, was partly soluble in sulphuric acid.

H. The residue marked (X F) was now thoroughly dry, and of a considerable hardness. Its colour was a glittering reddish brown. It was reduced to powder, and digested in liquid ammonia. This alkali, however, after having being examined, had taken up nothing. Concentrated sulphuric acid being presented to a small quantity of it, had likewise no effect upon it. It was therefore mixed with a small quantity of dry potash, and ignited for some time; then dissolved in distilled water, and transferred upon a filtre. The residue was repeatedly washed. After being considerably dry, it was put into a glass evaporating bason, pure sulphuric acid was poured over it, diluted with a little water, and the whole carefully evaporated to dryness. On being suffered to cool, and distilled water being added, part of it became dissolved, and another part remained insoluble in the form of a brownish powder, which was collected on a filtre.

The solution contained *iron*.

I. This sulphuric solution (H) was of a greenish hue; its taste was astringent. Prussiate of potash threw down prussiate of iron. Tincture of galls produced a black precipitate with it. Liquid ammonia occasioned a reddish brown precipitate, which, after being washed, dried, and ignited, weighed twelve grains. It was an *oxide of iron*. It was assayed with super-saturated borate of soda by means of the blow-pipe, for investigating the presence of manganese, but no vestige of this metal could be discovered.

The sulphuric solution, H. gave (by carbonate of potash) a precipitate of *zircone*.

K. The product obtained by means of sulphuric acid (H), was mixed with three times its own weight of carbonate of potash, and boiled in a sufficient quantity of distilled water. A light earthy substance became separated, tinged slightly yellow, by an admixture of ferruginous matter. Being disposed to believe that this earthy substance might be zircone, it was merely collected on a filtre and washed, but *not* ignited. Its weight was twenty-five grains. It would, if it had been ignited, probably weighed only twenty grains. This earth was insoluble in potash; but soluble in nitric and acetic acid.

United

United with fulphuric acid to perfect faturation, it yielded a falt of a difficult folubility; but if this acid was added in excefs, the falt was readily foluble in water. When ignited it became hard and infoluble in acids. I therefore do not hefitate to believe, that this earth was *zircone*, though not abfolutely pure, but foiled with iron.

From thefe experiments it follows that 200 grains of this hyacinth-like foffil, confift of

100	grains	filex (D)
56	——	alumine (G)
12	——	oxide of iron (I)
20	——	zircone (K)
12	——	Lofs of matter during the operation.
200		

XI.

Philofophical Difquifitions on the Proceffes of common Life.—Art of Shaving.—W. N.

THE caprice of fafhion, or the modern improvements in perfonal neatnefs, has deprived all the nations of Europe of their beards; and confequently a portion by no means inconfiderable of the fmall and daily conveniencies of life muft depend on the facility with which this appendage can be taken off. This fubject is frequently treated in converfation; and I am perfuaded that many individuals will be glad to know what can be ftated on the whole refpecting it. Procefs of fhaving.

The fabrication of a good razor depends on fo many circumftances and conditions, the material, the art of forging, the hardening and the temper, that the artift himfelf after he has exercifed his utmoft fkill, can in the laft inftance, felect fuch inftruments as are eminently fuperior to others, by trial or actual ufe. I am not aware of any means of chufing a good razor out of a number. All that I can fay in this refpect is, that a bad razor cannot be eafily brought to a fine, or even a moderate edge; for which reafon I fhould prefer that razor which poffeffes the beft edge, and has been moft flightly touched upon the hone: that is to fay, the razor which, upon looking Inftructions for chufing a good razor.

looking along its edge, has little or no flat part where the action of the hone has taken place, and which when drawn along the hand appears keen and ſmooth. I muſt here take notice, that the common practice of cutting the ſkin of the hand, in order to try a fine edge, is by no means ſo delicate as that of placing the edge of the razor lightly on the thick ſkin of the hand, ſo as to bear upon a length of about two inches, and then drawing it along through about one quarter of an inch, without cutting. In this manner the irregularities of edge in the fineſt ſurgeon's inſtruments may be plainly felt.

Character of a very fine edge.

Art of giving a fine edge to cutting tools.

I will ſuppoſe the operator to be in poſſeſſion of a good razor. He is in the next place to learn how to keep it in order. The original keenneſs of edge will neceſſarily go off by uſe. It can only be reſtored by whetting or grinding it up again. This is uſually done by means of a good ſtrap; in which little inſtruction is required beſides what is common to every operation upon the edge of cutting inſtruments; namely, that they muſt never be ſharpened but when actually dull, and that the operation of ſharpening them is miſchievous, if it be carried a moment beyond the time in which the full effect is produced. The act of ſtrapping produces a ſmooth edge; but on account of the elaſticity of the ſtrap this edge becomes round and obtuſe in the angle formed by its faces.

The ſtrap.

Whetting upon the hone.

A razor which has its edge too much rounded to be revived by the ſtrap, muſt be ſharpened upon the hone. We may conſider a hone as compoſed of fine ſand agglutinated together, the particles of which cut or raſp away the face of the tool that may be whetted upon it. This face will accordingly become filled with ſcratches, and the edge will be an irregular ſaw, with notches, ſo much the finer as the particles of ſand were ſmaller, and the preſſure of rubbing more light. It is found that the action of a hone is ſmoother, if it be left with oil upon the face when put by; an effect which probably ariſes from the oil becoming glutinous, and clogging the cutting points of the ſand. The principal inſtructions for whetting a razor are, 1. That it ſhould be drawn lightly along the ſtone by repeated alternate ſtrokes with the edge foremoſt; and by no means backwards and forwards, unleſs a conſiderable part be required to be whetted away, which can ſeldom be the caſe unleſs there be a notch, or manifeſt irregularity in

Particular inſtructions.

in the edge; 2. That the edge ſhould be tried upon the hand after every two or more ſtrokes, in order to aſcertain the inſtant at which it ſhall have become very uniformly rough. This roughneſs, if the hone be good, and the preſſure light, will conſtitute a very fine edge, though in general leſs ſmooth than is left by a good ſtrap; and 3. The edge will therefore be completed by a ſtroke or two upon ſuch a ſurface. But I will ſuppoſe that a longer continuance of whetting has been neceſſary, ſo as to produce what is called a wire, conſiſting of a very thin film of ſteel adhering along the edge. If this ſhould break off upon the hone during the whetting, the edge will become notched in paſſing over it; for which reaſon it muſt be taken off by a direct operation; namely, by paſſing the edge of the razor once along the hone, holding the back rather more upright than half way between the flat and the perpendicular poſition, and then paſſing it once back again, inclining the razor the contrary way. Theſe two ſtrokes will detach the wire, and produce a very perfect, though very obtuſe edge; which muſt then be brought up to the requiſite keenneſs by two or more light ſtrokes in the firſt mentioned method, taking care not to go too far. In both the proceſſes of ſtrapping and whetting, we have ſuppoſed the razor to be laid flat; but where a ſtrong edge is required, the whetting, but not the ſtrapping, may be performed with the back of the razor elevated ſomewhat leſs above the face of the hone, than half the breadth of the blade.

Wire upon the edge;

Taken off.

Our cutting inſtrument being now in order, there remains but one more obſervation before we proceed to the ſubject of operation; which is, that it will be found to act conſiderably better after immerſion in hot water. While I admit the fact, I muſt confeſs that I am far from being ſatisfied with any of the explanations. It has been ſaid, that the expanſion by heat enlarges the fine notches of the edge, and probably developes or opens others; but this effect can hardly be thought of any conſequence, when it is conſidered that the whole expanſion produced by boiling water does not exceed one part in ten thouſand. Others have remarked, that as heat ſoftens horn, hair, and other ſimilar ſubſtances, the hot edge may paſs through a hair more eaſily than if cold; but here we may remark, that the heat ſeems too little, and its application to the hair too momentary, to be productive of ſo conſiderable a difference;

Enquiry how far heating a razor in water is advantageous.

By improving the edge;

Or by ſoftening the hair;

difference; besides which the advantage is said, and I believe truly, to follow even if the razor be suffered to cool before it is used. Hence it should seem as if the hot water detached some particles of grease from the edge, and by that means facilitated the sliding of the cut surface of the hair upon the face of the razor.

Or by cleaning the blade.

Application and utility of soap.

There is much difference of opinion as to the application and use of soap. By some it is applied cold and thick by means of a brush; others apply it hot; and others again apply hot suds or soap-water, with much rubbing, until the alkali has rendered the skin much softer, and more disposed to be acted on by water than in its usual state. Sir John Chardin asserts, that the great excellence of the Persian barbers consists in this practice; but I can assert on the other hand, that the Chinese, who shave with exquisite facility, use a soap box and brush with cold water. Whether the effect of the soap be to soften the surface of the hair by an incipient combination of alkali; whether it renders the stroke more easy by causing the razor to slide with facility along the surface, instead of raising up portions of skin and cutting them off, as it might otherwise do, are questions to be solved by direct experiments. Of these I know none, excepting that a mere solution of alkali is less effectual than soap, and so likewise is the mere application of oil or fat. Hence probably we may infer, that soap acts in a twofold manner; by dissolving and removing the matter of perspiration by its alkali, and lubricating by its oil. With regard to the difference of heat and cold, these effects may perhaps be forwarded, and the hair somewhat softened at the higher temperature, though the difference seems to be not very considerable.

Persian and Chinese barbers.

Probable use of soap.

Mechanical process or operation of shaving.

Some operators place the razor flat upon the face, and others raise it to a considerable angle. It is certain that the process may be skilfully performed either way. It is a very bad practice to press the razor at all against the face; and indeed this can not be done with impunity if a drawing stroke be used. Unskilful shavers will generally injure the skin less if they lay the razor flat; but generally speaking, the closest shaving will be performed by holding the razor at the same inclination as was used in whetting it.

General Remarks.

Upon the intire view of the subject it appears to me, that the only indispensable condition among the requisites before discussed,

difcuffed, is that the razor fhould be in good order: and that all the other particulars may be varied, except the mechanical procefs of cutting;—that the great fecret of taking off the beard with facility, confifts in a drawing ftroke, that is to fay, that the line of the motion of the razor itfelf fhould be very oblique to the line of the edge, and not at right angles to that line, as is commonly practifed; fo that the tool may be made to exert its action as a faw, which is much more powerful than its fimple effect as a wedge. This method is indeed fo very effectual, as to require great care and confiderable practice before it can be adopted, in the extreme, with perfect fafety; but the fame efficacy which endangers the fkin, renders it eafy and pleafant with regard to the beard.

Effential part of manipulation.

XII.

Abftract of the Enquiries of CIT. BENEDICT-PREVOST, *and fome other Natural Philofophers, relative to the Motions of Odorous Subftances placed upon Water. By* CIT. BIOT *.

IT is a fact, which has long been known to natural philofophers, that fmall pieces of camphor, when placed upon water, have a very rapid rotatory motion. Volta and Brugnatelli have obtained the fame refults, by employing the benzoic and fuccinic acids. Citizen Benedict Prevoft has extended this property to a great number of odorous fubftances.

Motions of camphor, &c. upon water.

But however unanimous philofophers have been in admitting the facts, they differ greatly from each other in the explanations they have given.

Citizen Prevoft attributes thefe motions to the emanation of the odorous particles of the bodies: The experiments upon which he grounds this opinion may be found in the numbers that have been quoted. Venturi, Profeffor of Natural Philofophy at Modena, applies to thefe phenomena the explanation which Monge has given of the apparent attractions of fubftances floating upon the furface of water: according to him, "the water has a ftronger attraction for the folid camphor,

Difcuffions refpecting their caufe.

Venturi fuppofes the action of water.

* Soc. Philom. 54. The experiments of Prevoft, Venturi, and Carradori, alluded to in this notice, are given in the Philofophical Journal, 4to; for which fee the Indexes. N.

 than

than for the ſmall portion of it which it has already diſſolved and ſaturated; it aſcends along the ſolid piece, and there forms a curvilinear inclined ſurface. The ſmall portion that is diſſolved and ſaturated deſcends along this ſurface, and, whilſt it is deſcending, it puſhes backwards, according to the laws of mechanics, the ſurface itſelf, and the ſolid piece that adheres to it." He thinks that we ought not to confound this effect with the repulſions which air impregnated with ether, or the exhalations of very hot camphor, exerciſes upon the light bodies which are made to float upon the ſurface of the water. In this caſe only he admits the preſence of an elaſtic fluid. (*Annales de Chimie, tome* 21.)

Dr. Carradori, a kind of oil.

Dr. Carradori is of a different opinion: He explains this motion by the elective affinity of a ſpecies of oil, which, according to him, proceeds from the camphor in contact with the water. He believes that the retreat of the water, which takes place upon a wet china plate or piece of glaſs, when camphor or other odorous ſubſtances are placed upon it, is the effect of the elective attraction of the ſurface of the plate or glaſs for the oil which the ſubſtances emit; and, according to him, it is this oil which cauſes the water to recede, by ſubſtituting itſelf in its place. (*Annales de Chimie, tome* 37.) In ſupport of his opinion, Dr. Carradori alledges, that camphor does not move upon the ſurface of the water when this is very limited. He has not been able to make ſmall plates of metal move, as Citizen Prevoſt had announced, by placing a morſel of camphor upon them, and letting them float upon the ſurface of the water. However, I have ſeveral times repeated this experiment, and always with ſucceſs; but it requires a great deal of care and extreme accuracy.

New experiments and obſervations of Prevoſt.

Citizen Prevoſt has replied to Dr. Carradori, in a Memoir which he has addreſſed to the Society, and which is intitled, *Nouvelles Expériences ſur les Mouvemens ſpontanés de diverſes Subſtances, à l'approche ou au contacte les unes des autres.* The following are the principal facts which it contains:—

Ether acts at a diſtance.

A drop of ether, placed upon a ſmall plate of tin [*fer blanc*], 15 grammes (3½ drachms) in weight, throws it into a lively motion, though it does not touch the ſurface of this liquid.

Thus ether acts upon water *at a diſtance*. This fact may be verified in a very ſimple manner. If we place a ſmall plate of

of tin upon water, and bring the extremity of a glafs tube wetted with ether within fome centimetres diftance of it, the plate recedes.

Small pieces of camphor, thrown upon mercury that had been well dried, were agitated there with the fame motions as on the water. In order that this experiment may fucceed, it is neceffary that the mercury fhould be well cleaned or dried: the fmalleft particle of oil or fat fpread upon its furface ftops the motion. The pieces of camphor ought to be very fmall, the reafon of which will be feen hereafter. **Camphor upon dry mercury.**

Very thin plates of mica, placed upon mercury, and charged with a fmall piece of camphor, move in the fame manner as upon water. **Camphor upon mica floating on mercury.**

The benzoic acid likewife affumes a rotatory motion upon mercury; but it is neceffary that it be reduced into almoft imperceptible fragments. An oily aureola forms itfelf round thefe fragments. Nothing fimilar to this is obferved round camphor, not even when examined with the microfcope. The metallic luftre of the mercury is not impaired by it. **Benzoic acid.**

It refults from thefe facts, that the prefence of water is not neceffary to the motions of odorous fubftances. **Water unneceffary.**

Thefe fubftances caufe the water to recede upon plates of alum, of pottery [*terre à faiance*], and of gum arabic, in the fame manner as upon a wet plate of porcelain. This retreat is not therefore owing to the elective attraction of the oily or odorous fubftance for the furface of the plate.

Finally, notwithftanding the affertion of Dr. Carradori, camphor moves in very confined veffels: Citizen Prevoft has feen it agitated in capillary tubes, into which it was introduced in very minute fragments.

From thefe facts Citizen Prevoft concludes, that the intervention of an elaftic fluid is neceffary to the production of thefe phenomena. To the facts which he has adduced, I fhall fubjoin the following, which appear to me to decide the queftion relative to the motions of camphor upon water. **Inference, that the motion is caufed by an elaftic fluid.**

If we cut a fmall piece of camphor, of the weight of a few grains, into the form of a cone, and approach it to the diftance of four or five millemetres of a very fmall piece of leaf gold floating upon the furface of the water, prefenting the point to it, this fmall piece of gold is repelled; and we may thus conduct it through the whole extent of the vafe, without ever being **_Experimentum crucis._**

being able to touch it. It is neceffary that the water fhould be very pure, and the veffel perfectly clean. We may hold the piece of camphor with a pair of tweezers, or at the end of a glafs tube; it muft be cut into a conical form, as has been mentioned; a piece of a larger fize and an irregular form would envelope the light body in its atmofphere, and it would not move with equal facility.

The fame effects are produced, if we employ, inftead of camphor, a fmall piece of fine fponge foaked in camphorated water, or merely a tube of glafs, containing at its extremity a drop of the fame folution.

Variation. If we cover a china plate with a very thin layer of water, and approach the piece of camphor of the preceding experiment within a few millemetres of it, prefenting it by the point, in fuch a manner that the axis of the cone is perpendicular with the furface of the layer, the water recedes under the cone, and forms a circle concentric with it. The interior part of this circle is coloured with prifmatic ranges, proceeding from the prolongation of the axis, and extending from the centre towards the circumference with a very rapid motion; after fome moments, the circle lofes its colour from the centre to the circumference, and the iris finally difappears, whether we continue to hold the camphor over the furface of the layer or not. It is indifferent whether we hold the plate in a horizontal or vertical pofition: the circle is always formed in a direction perpendicular to the axis of the fmall cone of camphor. I obferved thefe phenomena at the temperature of 15° of Reaumur's thermometer.

Ether on water. Laftly, if we throw a fmall piece of fine fponge foaked in ether upon water, it is inftantaneoufly thrown into motion like camphor; and a hiffing noife is heard, fimilar to that of water during its converfion into vapour upon a hot iron. If we view the furface of the water horizontally, before a light window, we obferve fparkling jets iffuing from the fponge, which extend themfelves in ferpentine windings upon the furface of the water, to the diftance of fome centimetres, and there produce irides fimilar to thofe of the preceding experiment. Thefe irides foon difappear. During this emiffion, the fponge has a progreffive and a rotatory motion, which are evidently owing to thofe fmall jets, the impulfe of which it is obferved conftantly to obey.

Of

Of these three experiments, the two first shew that camphor acts upon water *at a distance*, and without contact; the third renders the manner in which its motions can be performed upon this liquid perceptible to the senses. Inferences.

It appears to me, that from a comparative view of these facts, we may deduce the following inferences as certain:

Camphor is moved upon the surface of water by the effect of the emission of the particles which compose it; an emission that becomes perceptible to our senses by the smell which it produces, and by the repulsions which it exercises against small bodies floating upon the surface of the water.

As the effect resulting from these different impulses does not pass through the centre of gravity of the piece of camphor, this centre has a progressive motion, and the body revolves round it. As the figure of the piece of camphor changes every moment, the motion of its centre of gravity is neither uniform nor rectilinear; it varies incessantly, as well as the angular velocity of rotation. As the evaporation takes place principally at the surface of the water, the rotatory motion establishes itself round the axis which is perpendicular to this surface, and which passes through the centre of gravity of the body.

As, *ceteris paribus*, the emanation of the particles of the camphor is proportionate to the extent of its surface, and as the surfaces increase only as the squares, but the masses as the cubes of corresponding dimensions, the rapidity of the motion of the camphor must be greater in proportion as its volume is smaller, and consequently its motion must be accelerated in proportion as it evaporates; which coincides with experience.

Having established these propositions, which in my opinion comprehend the true theory of the motions of camphor upon water, let us return to the second part of the work of Citizen Benedict Prevost.

It includes a great number of experiments, in which we see inodorous substances produce, upon wet glass, the same phenomena as odorous, oily, or volatile substances. Other experiments with bodies not odorous.

If we spread a small piece of fine wet linen, of any figure, upon a china plate moistened with a thin layer of water, the water appears to recede all round it, forming a multitude of jets, and exhibiting the prismatic colours *.

* These colours probably result from the decomposition of the light by the small layer of water which surrounds the piece of linen, and which becomes still thinner by the retreat of the water.

If

If we pour upon the piece of linen, after having ſpread it upon the plate, ſome drops of water coloured with logwood, this water runs off in coloured ſtreaks.

The ſame effects take place when we uſe a piece of fine white paper, not ſized.

We obtain them equally with all the animal and vegetable ſubſtances, with the ſaline liquids and ſolutions; whether we place them, under the ſame circumſtances, in contact with each other, or with water.

Theſe phenomena take place not only upon a wet china plate, but they are alſo obſerved upon ſurfaces of pottery, and many other ſubſtances.

From theſe facts, and ſeveral other's analogous to them, Citizen Prevoſt draws the following inferences:

General reſults. 1. That all liquids poſſeſs the property of mutually repelling each other.

2. That all dry organiſed ſubſtances, which preſerve any remains of organiſation, exhale, whilſt they imbibe water, an elaſtic fluid, which carries with it a part of this water, and repels that which ſurrounds it upon a piece of wet glaſs.

The firſt inference is contradictory to the general law of the mutual attraction of the molecules of matter.

As to the hypotheſis of Citizen Prevoſt, concerning the formation of an elaſtic fluid, we ſhall remark, that, before he attributes phenomena to new cauſes, he ought to endeavour to account for them by thoſe which are already known; to diſtinguiſh the effects produced by odorous ſubſtances from thoſe preſented by inodorous ones; and perhaps to eſtabliſh in a more certain manner this repulſion of liquids by paper and linen; for this repulſion may very poſſibly be nothing more than an appearance produced by the flowing down of the water upon the inclined ſurface which theſe ſubſtances raiſe around themſelves as they imbibe this liquid.

XIII.—Caſe

XIII.

Case of a young Gentleman, who recovered his Sight when ſeven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. James Ware, *Surgeon.* *

History of the infancy of a child blind with cataracts.

MASTER W. the ſon of a reſpectable clergyman, at Caſtlecary, in Somerſetſhire, was born in the year 1793; and, for many months, appeared to be a healthy perfect child: his eyes, in particular, were large and rather prominent. When about ſix months old, he began to cut his teeth; which was attended with great pain, and frequently with violent convulſive fits. About the end of his firſt year, a number of perſons paſſing in proceſſion near his father's houſe, accompanied with muſic and flags, the child was taken to ſee them; but, inſtead of looking at the proceſſion, it was obſerved that, though he was evidently much pleaſed with the muſic, his eyes were never directed to the place from whence the ſound came. His mother, alarmed by this diſcovery, was naturally led to try whether he could ſee ſilver ſpoons, and other glaring objects, which ſhe held before him at different diſtances; and ſhe was ſoon convinced, that he was unable to perceive any of them. A ſurgeon in the country was conſulted, who, on examining the child's eyes, diſcovered an opacity in the pupils, which was ſo conſiderable, that he did not heſitate to pronounce there was a complete cataract in each. A deſcription of the child's ſituation was then ſent to me, with a requeſt that I would point out thoſe ſteps which its parents ſhould purſue. The caſe was ſo evident, that I could not heſitate in ſaying, that the removal of the opaque cryſtalline humour, from the place it occupied behind the pupil, was the only method by which the child could obtain his ſight; and, attached as I was, at that time, in all caſes, to the operation of extracting the cataract, in preference to that of depreſſing it, I added, that I did not think he would be fit for the operation, until he was at leaſt thirteen or fourteen years old. This advice being approved, all thoughts of aſſiſting his ſight were, for the preſent, relinquiſhed. He

* Philoſ. Tranſ. 1801, p. 382.

ſoon

History of the infancy of a child blind with cataracts.

foon difcovered a great fondnefs for mufic; his memory was very retentive of the little ftories that were read or recited to him; and, in every way, it became evident that he had a mind capable of receiving information. As foon as he could fpeak, it was alfo obferved, that when an object was held clofe to his eyes, he was able to diftinguifh its colour, if ftrongly marked; but, on no occafion, did he ever notice its outline or figure. In November, 1800, his parents took him to Briftol; whither they went for the purpofe of feeing the works carried on in the fchool for the indigent blind in that city, and in order that they might afcertain whether their fon, who was then arrived to his feventh year, could be taught any thing that would be ufeful or amufing. Here he very quickly learnt the art of making laces. But his parents, having brought him fo far from home, thought it advifeable to extend their plan, and make a vifit to the metropolis, for the fake of giving me an opportunity of infpecting his eyes, and of hearing whether my opinion continued the fame as that which I had written to them fix years before. About a month previous to the time of their arrival, a Portuguefe boy, fourteen years old, had been put under my care, who was in a fimilar fituation; and, in this cafe, notwithftanding all the efforts I could ufe, I found it impoffible to fix the eye, in order to extract the cataract, without employing a degree of force which might have been highly injurious. I therefore relinquifhed my intention of performing the operation in that way, and determined to make ufe of the couching needle; being prepared, either to deprefs the cataract with this inftrument, if it was fufficiently folid for the purpofe, or, if it was foft or fluid, (which I rather expected,) to puncture its capfule largely, fo as to bring the opaque cryftalline into free contact with the aqueous and vitreous humours. In order to fix the eye for this operation, I was not afraid to make ufe of a fpeculum oculi; fince a preffure, which would have been highly dangerous in extracting the cataract, might be applied on the prefent occafion with perfect fafety. Conformably to my expectation, the cataract was of a foft confiftence; in confequence of which, I was not able to deprefs it, and contented myfelf with making a large aperture through the capfule, by means of which the cryftalline was brought into contact

Cafe of a boy who recovered his fight by puncture of the capfule of the cryftalline.

contact with the other humours, a confiderable part of it coming forwards, and fhewing itfelf directly under the cornea.

This being the immediate refult of the operation, it could not be expected that any improvement fhould be made in the fight of the patient at that time. In a few days, however, the opaque matter was wholly abforbed; the pupils became clear; and the lad recovered the fight of both his eyes.* Encouraged by the fuccefs which followed this operation, I was induced to retract the opinion which I had formerly fent to Mafter W.'s father, (which opinion I had given under the impreffion that the cataract fhould be extracted,) and I now propofed, that an attempt fhould be made to afford relief to one eye, at leaft, without further lofs of time; this attempt, in the way above mentioned, being practicable with as much fafety at his prefent age as at any future period; and, if it proved fuccefsful, it would give the young gentleman the benefit of vifion five or fix years fooner than his friends had been encouraged to expect, by my former letter on this fubject. They were naturally much pleafed with this alteration in my advice; and the child himfelf appearing to poffefs a great degree of fortitude, I performed the operation on the left eye, on the 29th of December laft, in the prefence of Mr. Chamberlayne, F. A. S. Doctor Bradley, of Baliol College, Oxford, and Mr. Platt, furgeon, in London. It is not neceffary, in this place, to enter into a defcription of the operation. It will be fufficient to fay, that the child, during its performance, neither uttered an exclamation, nor made the fmalleft motion, either with his head or hands. The eye was immediately bound up, and no inquiries made on that day with regard to his fight. On the 30th, I found that he had experienced a flight ficknefs on the preceding evening,

The fame operation performed on the fubject of this paper, at about eight years old.

* It fhould be remarked, that the fight obtained by children who are born with cataracts, is feldom fo perfect as that which thofe recover, after the operation, who are afflicted with the diforder later in life. In confequence either of fome remaining opacity in the cryftalline capfule, which hinders the free admiffion of the rays of light, or of a greater tenuity in the remaining humours of the eye, children require, in general, a much deeper convex glafs to enable them to fee minute objects; and, at the fame time, they are obliged to hold them much nearer their eyes than older perfons.

but

but had made no complaint of pain, either in his head or eye. On the 31ſt, as ſoon as I entered his chamber, the mother, with much joy, informed me that her child could ſee. About an hour before my viſit, he was ſtanding near the fire, with a handkerchief tied looſely over his eyes, when he told her that under the handkerchief, which had ſlipped upward, he could diſtinguiſh the table by the ſide of which ſhe was ſitting: it was about a yard and a half from him; and he obſerved that it was covered with a green cloth, (which was really the caſe,) and that it was a little farther off than he was able to reach. No further queſtions were aſked him at that time; as his mother was much alarmed, leſt the uſe thus made of his eye might have been premature and injurious. Upon examination, I found that it was not more inflamed than the other eye; and the opacity in the pupil did not appear to be much diminiſhed. Deſirous, however, to aſcertain whether he was able to diſtinguiſh objects, I held a letter before him, at the diſtance of about twelve inches, when he told me, after a ſhort heſitation, that it was a piece of paper; that it was ſquare, which he knew by its corners; and that it was longer in one direction than it was in the other. On being deſired to point to the corners, he did it with great preciſion, and readily carried his finger in the line of its longeſt diameter. I then ſhewed him a ſmall oblong band-box covered with red leather, which he ſaid was red and ſquare, and pointed at once to its four corners. After this, I placed before him an oval ſilver box, which he ſaid had a ſhining appearance; and, preſently afterwards, that it was round, becauſe it had not corners. The obſervation, however, which appeared to me moſt remarkable, was that which related to a white ſtone mug; which he firſt called a white baſon, but, ſoon after, recollecting himſelf, ſaid it was a mug, becauſe it had a handle. Theſe experiments did not give him any pain; and they were made in the preſence of his mother, and of Mr. Woodford, a clerk in his Majeſty's Treaſury. I held the objects at different diſtances from his eye, and inquired very particularly if he was ſenſible of any difference in their ſituation; which he always ſaid he was, informing me, on every change, whether they were brought nearer to, or carried further from him. I again inquired, both of his mother and himſelf, whether he had ever, before this

He gained his ſight,

and immediately diſtinguiſhed a table and *its diſtance*;

alſo *ſquare paper*;

a *round* ſhining *box*;

and a white *mug*.

Other experiments, by which it appeared that he knew diſtances and figures with much preciſion;

this time, diſtinguiſhed by ſight any ſort of object; and I was aſſured by both that he never had, on any occaſion; and that, when he wiſhed to diſcover colours, which he could only do when they were very ſtrong, he had always been obliged to hold the coloured object cloſe to his eye, and a little on one ſide, to avoid the projection of the noſe. No further experiments were made on that day. On the 1ſt of January, I found that his eye continued quite free both from pain and inflammation, and that he felt no uneaſineſs on the approach of light. I ſhewed him a table knife; which at firſt he called a ſpoon, but ſoon rectified the miſtake, giving it the right name, and diſtinguiſhing the blade from the handle. by pointing to each as he was deſired. He afterwards called a yellow pocket-book by its name, taking notice of the ſilver lock in the cover. I held my hand before him; which he knew, but could not at firſt tell the number of my fingers, nor diſtinguiſh one of them from another. I then held up his own hand, and deſired him to remark the difference between his thumb and fingers; after which, he readily pointed out the diſtinctions in mine alſo. Dark-coloured and ſmooth objects, were more agreeable to him than thoſe which were bright and rough. On the 3d of January, he ſaw, from the drawing-room window, a dancing bear in the ſtreet; and diſtinguiſhed a number of boys that were ſtanding round him, noticing particularly a bundle of clothes which one of them had on his head. On the ſame evening, I placed him before a looking glaſs, and held up his hand: after a little time he ſmiled, and ſaid he ſaw the ſhadow of his hand, as well as that of his head. He could not then diſtinguiſh his features; but, on the following day, his mother having again placed him before the glaſs, he pointed to his eyes, noſe, and mouth, and ſeemed much gratified with the ſight.

a dancing bear;

looking glaſs.

Having thus ſtated the principal obſervations that were made by Maſter W. I ſhall now make a brief compariſon between this ſtatement, and that which is given in the XXXVth volume of the Philoſophical Tranſactions, of Mr. Cheſelden's patient, who was ſuppoſed to be born blind, and obtained his ſight when he was between thirteen and fourteen years old.

Compariſon of this ſtatement with the famous inſtance of Cheſelden.

It

It ſhould be obſerved, that though Maſter W. was ſix years younger than Mr. Cheſelden's patient, he was remarkably intelligent, and gave the moſt direct and ſatisfactory anſwers to every queſtion that was put to him. Both of them, alſo, if not born blind, loſt their ſight ſo very early, that, as Mr. Cheſelden expreſſes it, "they had not any recollection of having ever ſeen."

My firſt remark is, that, contrary to the experience of Mr. Cheſelden's patient, who is ſtated "to have been ſo far from making any judgment of diſtance, that he thought all objects touched his eyes, as what he felt did his ſkin." Maſter W. diſtinguiſhed, as ſoon as he was able to ſee, a table, a yard and a half from him; and proved that he had ſome accuracy in his idea of diſtance, by ſaying, that it was a little further off than his hand could reach. This obſervation, ſo contrary to the account we have received of Mr. Cheſelden's patient, would have ſurpriſed me much more than it did, if I had not previouſly, in ſome ſimilar inſtances, had reaſon to ſuſpect that children, from whom cataracts had been extracted, had a notion of diſtance the firſt moment they were enabled to ſee. In the inſtance particularly of a young gentleman from Ireland, fourteen years old, from each of whoſe eyes I extracted a cataract, in the year 1794, in the preſence of Dr. Hamilton, phyſician to the London Hoſpital, and who, before the operation, aſſured me, as did his friends, that he never had ſeen the figure of any object, Dr. Hamilton and myſelf were much aſtoniſhed by the facility with which, on the firſt experiment, he took hold of my hand at different diſtances, mentioning whether it was brought nearer to, or carried further from him, and conveying his hand to mine in a circular direction, that we might be the better ſatisfied of the accuracy with which he did it. In this caſe, however, and in others of a like nature, although the patients had been certainly been blind from early infancy, I could not ſatisfy myſelf that they had not, before this period, enjoyed a ſufficient degree of ſight to impreſs the image of viſible objects on their minds, and to give them ideas which could not afterwards be entirely obliterated. In the inſtance of Maſter W. however, no ſuſpicion of this kind could occur; ſince, in addition to the declaration of himſelf and his mother, it was proved by the teſtimony of the ſurgeon

Other inſtances differing from Cheſelden.

who examined his eyes in the country, that the cataracts were fully formed before he was a year old. And I beg leave to add further, that on making inquiries of two children, between feven and eight years of age, now under my care, both of whom have been blind from birth, and on whom no operation has yet been performed, I find that the knowledge they have of colours, limited as it is, is fufficient to enable them to tell whether coloured objects be brought nearer to, or carried further from them, for inftance, whether they are at the diftance of two inches or four inches from their eyes; nor have either of them the flighteft fufpicion, as is related of Mr. Chefelden's patient, that coloured objects, when held before them, touch their eyes.

But the judgment which Mafter W. formed of the different diftances of objects, was not the only inftance in which he differed from Mr. Chefelden's patient; who, we are informed, "did not know the figure of any thing, nor any one thing from another, however different in fhape and magnitude;" for Mafter W. knew and defcribed a letter, not only as white, but alfo as fquare, becaufe it had corners; and an oval filver box, not only as fhining, but alfo as round, becaufe it had not corners: he likewife knew, and called by its name, a white ftone mug, on the firft day he obtained his fight, diftinguifhing it from a bafon, becaufe it had a handle. Thefe experiments were made in the prefence of two refpectable perfons, as well as myfelf; and they were feveral times repeated, to convince us that we could not be miftaken in them. I mention the circumftance, however, with much diffidence, being aware that the obfervations not only differ from thofe that are related of Mr. Chefelden's patient, but appear, on the firft ftatement, to oppofe a principle in optics, which I believe is commonly and juftly admitted, that the fenfes of fight and feeling have no other connection than that which is formed by experience; and, therefore, that the ideas derived from feeling can have no power to direct the judgment, with refpect either to the diftance or form of vifible objects. It fhould be recollected, however, that perfons who have cataracts in their eyes, are not, in ftrictnefs of fpeech, blind, though they are deprived of all ufeful fight. The inftances I have adduced prove, that the knowledge they have of colours is fufficient to give them fome idea of diftance, even in their

Remarks on figures, &c.

darkeft

darkeſt ſtate. When, therefore, their ſight is cleared by the removal of the opaque cryſtalline, which intercepted the light, and the colour of objects is thereby made to appear ſtronger, will it be difficult or unphiloſophical, to conceive that their ideas of diſtance will be ſtrengthened, and ſo far extended as to give them a knowledge, even of the outline and figure of thoſe objects with the colour of which they were previouſly acquainted?

Chirurgical obſervations and remarks.

The caſe which I have here related appears to deſerve notice, not only on account of the obſervations that were made by the patient on recovering his ſight, but alſo on account of the hint which it affords to ſurgeons, relative both to the mode in which the cataract may beſt be removed, when children are born with this diſorder, and the time when it is moſt proper to perform the operation.

In what caſes depreſſion of the cataract may be preferable to extraction.

The Baron de Wenzel, in his ingenious Treatiſe on the Cataract, with great force of reaſoning, deduced from the long and ſucceſsful experience of his father and himſelf, recommends, in all caſes of this diſorder, without making any exceptions, the operation of extraction, in preference to that of depreſſion; and I believe it is now generally acknowledged by medical men, that in the more common caſes, his deciſion as to the mode of operating is perfectly well founded. The Baron admits that the operation is not ſo certain a cure in children as it is in perſons of a more advanced age; both on account of their untractableneſs, and becauſe, in them, the opacity of the cryſtalline is not unfrequently accompanied with an opacity in the capſule that contains it. On theſe accounts, when children are born with this diſorder, he adviſes to poſtpone the operation, until they are old enough to be made ſenſible of the loſs they ſuſtain by the want of ſight, and have firmneſs of mind to ſubmit patiently to the means that are requiſite in order to obtain it. Influenced by this opinion of the Baron, and believing the operation of extraction to be ſo much ſuperior to that of depreſſion, that the latter ought not, on any occaſion, to have the preference, I have given advice, in the caſes of a conſiderable number of children who were born with this diſorder, to poſtpone every attempt to relieve them, until they were thirteen or fourteen years old. Prior to this time, it did not appear to me that children could be depended upon to ſubmit, with due ſteadineſs, to the repeated introduction of

inftruments, which is fometimes neceffary in extracting the cataract; and, even at this age, the eyes of fome are fo fmall, and in fuch a conftant rolling motion, that it is almoft impoffible properly to accomplifh the operation. The Portuguefe lad, whofe cafe has been related, afforded an inftance of this kind; and I confider it as a fortunate circumftance that it came under my notice, fince, in fome degree, it may be faid to have obliged me to examine, more attentively than I had before done, the advantages and difadvantages of the operation of depreffion; which operation, being more eafy to perform than that of extraction, has certainly this advantage in the cafes of children, (to which alone I here advert,) that it may be performed with equal fafety when they are only feven years of age, as it may at any fubfequent period of their lives.

Pott favoured the practice of depreffion.

It is well known that the late Mr. Pott, who publifhed his remarks on the cataract in the year 1775, was a ftrenuous advocate for this operation; and, though he appears to me to have much under-rated the advantages of extraction, it muft be allowed that he makes many juft and highly pertinent obfervations on the ufe of the couching needle, in thofe cafes where the cataract is foft, or fluid. Mr. Pott confidered this as a very common ftate of the diforder; and does not make any diftinction between the cataract when it attacks grown perfons, and when children are born with it. In the former cafe, experience inclines me to believe, that the cataract is very rarely fluid, or even foft; whereas, in the latter, I have always found it, agreeable to the obfervation of the Baron de Wenzel, in one or other of thefe ftates. Although, therefore, in the cafe of grown perfons, the operation of extraction appears to me to have very great advantages over that of depreffion, yet, in the cafe of children, I can readily accede to almoft the whole that Mr. Pott advances in favour of depreffion. If the couching needle be paffed in the way in which it is ufually introduced to deprefs the cataract, and thereby a large aperture be made in the capfule of the cryftalline, (which operation may be performed with perfect fafety, and with very little pain to the patient, whilft the eye is fixed with a fpeculum oculi,) the opaque cryftalline, being thus brought into contact with the aqueous and vitreous humours, will, in a fhorter or longer fpace of time, according to its degree of foft-

Reafons in fupport of Pott.

preceding pages, in the presence of Mr. Scott, surgeon, in St. Alban's-street. The operation gave her no pain; and, in the course of a few days, the opacity was evidently diminished, particularly in the right eye, the patient discovering the colour of objects more plainly than before, but being still unable to distinguish their figure. At the end of a month, finding no further improvement in her vision, it appeared to me most probable that the remaining opacity was situated in the capsules. I therefore determined to extract either a part or the whole of each of them. The incisions of the cornea were made in the usual manner; after which, I punctured the anterior parts of both the capsules with the sharp end of a gold curette. The punctures became immediately transparent, without affording an issue to the liquor Meibomii, or any other humour. From hence it seems evident, that nothing was contained within the capsules, or, in other words, that the crystalline humours were absorbed; and it appears to me highly probable, that their absorption had been occasioned by the previous operation of puncturing their capsules with the couching needle. I dilated the new punctures with the end of the curette; and afterwards, being still afraid that the apertures in the capsules might not be large enough to admit a sufficient number of the rays of light, I removed a portion of each of them with a small forceps. This was accomplished in the left eye, without occasioning the discharge of any part of the vitreous humour; and, in the right, the quantity of this humour that came away was very small. In the course of a week, the inflammation that followed the operation was nearly removed; a large portion of both pupils was quite clear; and the young lady distinguished objects with quickness and precision.

SCIENTIFIC

SCIENTIFIC NEWS.

NATIONAL INSTITUTE OF FRANCE.

Extract of the Report of the Transactions of the Class of Mathematical and Physical Sciences, during the last Trimestre of the Year 9. French National Institute.

Mathematical Part. *By* CITIZEN DELAMBRE.

Memoir on the Equilibrium of Arches. *By* CIT. BOSSUT. Bossut on arches.

AS the key stones of which an arch is composed, support themselves by mutual equilibration, and remain suspended without the aid of any support beneath, their whole force directing itself against the buttresses; in order to ensure the duration of an arch, it is not sufficient that all its key-stones be in equilibrio; but it is necessary that its supports at the ends should oppose a resistance adequate to the force which it exerts in order to overthrow or break them in pieces.

The enquiry respecting the means proper for preventing their being overthrown, constitutes what is termed the problem of the lateral pressure of arches. Several geometricians of the last century had already occupied themselves with this subject, but they had intirely neglected to inquire into the means of preventing these supports from being crushed.

C. Bossut undertook, in the year 1770, to treat the question in the most general manner, as well for simple arches as domes. He has investigated whatever relates to the figure and the lateral pressure of vaults. His memoirs have been printed in the volumes of the Academy of Sciences for the years 1774 and 1776.

After many new observations and various experiments, which may be of the greatest practical utility, C. Bossut has resumed the subject of his two memoirs. He has new modelled them by simplifying his calculations in several places, and has made a number of additions relative both to theory and practice; so that the whole, in its present state, constitutes a work that may be considered as original.

Comets.—C. Messier has read an account of the comet which he discovered on the 23d of Messidor last, about half an hour Comet of July 12, 1801.

hour after eleven in the evening: its light was very feeble; in 41 minutes of time it moved 24 min. 40 fec. of right afcenfion direct, and 6 min. 38 fec. north declination decreafing. The fame comet was feen on the fame day, and almoft at the fame inftant by Citizens Mechain and Bouvard; the latter of whom had even obferved it at the meridian at $11^h\ 57^m$ 49 fec. true time. The right afcenfion was 111 deg. 15 min. and the north declination 69 deg. 30 min.

C. Pons obferved it on the fame day at Marfeilles, and he had even perceived it on the preceding day; but the clouds did not at that time permit him to afcertain, by regular obfervations, whether it was a comet, or merely a nubecula.

Summer folftice, 1801.

Obfervation of the fummer folftice of the year 9.—Citizen Duc-la-Chapelle, affociate, has communicated to us the refult of the obfervations, made by him at Montauban, in order to determine the folftitial height of the fun, and the obliquity of the ecliptic.

From an average of nine days obfervations, he finds 23 deg. 28 min. 9 fec. for the apparent obliquity, taking 15 min. 48 deg. for the femi-diameter of the fun, and 44 deg. 0 min. 52 fec. for the latitude of his obfervatory.

Lalande on the fixed ftars.

Remarks concerning the 50,000 *ftars, of which obfervations have been publifhed by Citizen Jerom Lalande.*—C. Lalande, in the preface to his *Hiftoire Célefte*, had noticed the exiftence of many blank fpaces in the heavens, many changeable ftars, and many red ftars. He now refumes thefe fubjects in a more circumftantial manner, in a memoir accompanied with tables.

By blank fpaces, (*d'efpéces vuides*) Citizen Lalande underftands in this memoir, thofe fpaces in which no ftars of the 9th degree of magnitude are to be perceived. Thefe are the fmalleft that can eafily be diftinguifhed with an achromatic telefcope of 67 millimeters aperture, in which light is admitted to illuminate the wires.

It is not to be doubted that by excluding all light from furrounding objects, and employing the moft powerful telefcopes, the blanks properly fo called would be found to be confiderably diminifhed; perhaps even there is not a fingle fpot in the firmament, towards which we could direct a telefcope, without perceiving a great number of ftars, though below the ninth degree of magnitude, and confequently too fmall to be of any ufe in aftronomy. C. Lalande gives the catalogue of

all thefe blank fpaces; that is to fay, his table comprehends the right afcenfion and declination of the middle of each of thefe fpaces.

The changeable ftars are comprehended in a fecond table; they are thirty one in number. There are not more than twelve, the periods of which are known; but there are feveral others which are diminifhed fo as to difappear. By following them attentively, we may determine the time which elapfes between two fucceffive difappearances; and this is a fpecies of obfervation which Citizen Lalande propofes to the curiofity of thofe who, having only indifferent inftruments, wifh neverthelefs to be ufeful to aftronomical fcience.

A third table prefents thirty-three ftars of a red colour. Since the year 1756, Mayer had remarked this colour in the 19th of Pifces, which he defignates in his regifters of obfervations by the epithet of *Rubicunda*, as he finds by the copy, which Prof. Lichtenberg has fent of all the obfervations made by Mayer, on the day when he obferved Herfchel's planet. Mitchell and Baillie fuppofe, that the colours of the ftars may depend either on the different intenfity of their fire, or the degree of their inflammation, and that the red colour may indicate a fire which is in a diminifhing ftate. According to this hypothefis, it would be a matter of importance to examine the changes of colour which take place in the ftars. Be that as it may, thefe variations, if they exift, are certainly extremely flow; for the different fhades of colour which are remarked at the prefent day in Antares, Arcturus, Aldebaran, Sirius, and the Lyre, exifted in the age of Ptolemy.

Extract of a memoir on the degree of magnetifm which blades of fteel of different thicknefs acquire, and on fome refults relative to the needles of the compafs, by CITIZEN COULOMB.—Almoft all the magnetic phenomena may be reduced to calculation, if we fuppofe two magnetic fluids to exift in the fteel, in each of which the molecules repel each other in the inverfe ratio of the fquare of the diftances, and attract the particles of the other fluid in the fame ratio. This law has been proved by C. Coulomb in the Memoires of the Academy of Sciences for 1786, according to experiments which appear to be decifive. Coulomb on magnetifm.

When the fteel is in its natural ftate, and has not been touched with the load ftone, the two fluids are neutralized by each other; that is to fay, they keep themfelves in equilibrio, and

and exert no action, since one of the fluids attracts a magnetic particle with the same force as that with which the other repels it.

When the fluids are separated, they tend to unite and neutralize each other; and they would actually unite, were there not a coercive force that opposed this union. This force may be either the adhesion of the molecules of the fluid to the steel, or the friction which they undergo in passing from one point to another.

The author has proved, *Memoires de l'Academie des Sciences pour* 1787, page 491, that whether we suppose the two fluids to be separated and carried each to one extremity of the blade, or that they are only displaced in each particle of the steel, the result of the calculation will be the same. He has proved at the same time, that this latter supposition is the only one which can be made to agree with the facts.

In the present memoir the author endeavours to determine the magnetic state of several blades united successively one above the other; or, which amounts to the same thing, he endeavours to determine the powers of different blades relative to their thickness.

New Planet.

New planet between Mars and Jupiter.

Mr. Piazzi, of the University of Palermo, discovered on the 1st of January, 1801, a star which appears to be a new planet. By observations repeated for several succeeding days he concluded, that its orbit is not likely to be parabolic, but agrees best with the hypothesis of a circle, the radius of which appears to be 2,6362 of the earth's mean distance, and consequently its position will be between Mars and Jupiter. Its bulk appears to be about $1\frac{1}{4}$ that of the earth. He has assigned to it the name of *Ceres Ferdinandia*, being the names of the ancient divinity of Sicily, and of its present sovereign the founder of the Observatory at Palermo. An account has been lately presented to the Royal Society. At present I give the following particulars from a paper in German.

OBSERVATIONS.

OBSERVATIONS.

1801	Med. Time. h m	Geo. Long. ° ′	Geo. Lat. ° ′	Right As. ° ′ ″	Decl. ° ′ ″
June 20	13. 4	101. 45	3. 26 N	103. 6 40	26 22 10 N
July 17	1. 43	113. 3	4. 6	115. 38 50	25 32 35
Aug. 12	10. 54	124. 21	4. 51.	127. 56 5	23 51 10
Sept. 7	16. 19	135. 23	5. 41	139. 44 30	21 38 25
12	22. 0	137. 40	5. 52	142. 2 20	21 8 10
18	3. 0	139. 50	6. 3	144. 17 0	20 37 0
23	8. 0	141. 58	6. 15	146. 29 20	20 6 25
28	13. 0	144. 5	6. 27	148. 40 0	19 35 0
Oct. 3	17. 41	146. 9	6. 40	150. 47 0	19 4 40
8	22. 0	148. 12	6. 53	152. 50 50	18 33 40
14	3. 0	150. 12	7. 8	154. 55 30	18 4 40
19	7. 0	152. 11	7. 22	156. 56 20	17 34 20
24	11. 0	154. 8	7. 37	158. 55 0	17 5 10
29	14. 45	156. 3	7. 53	160. 51 30	16 37 0
Nov. 3	18. 0	157. 56	8. 9	162. 45 40	16 9 0
8	22. 0	159. 48	8. 26	164. 38 50	15 41 50

	S. ° ′ ″
Place of Afcending Node - - - - - -	2. 20. 58. 30
Inclination of the Orbit - - - - - - - -	10. 47. 0
Place of the Aphelion - - - - - - - - -	2. 8. 59. 37
Time of paffage through the Aphelion Jan. 1. 1801 - - - - - - - - - - - - -	1,3328
Excentricity - - - - - - - - - - - -	0,0364
Log. of the half great Axis - - - - - - -	0,4106986
Time of the Siderial period - - - - - - -	4,13 years.

ACCOUNT OF BOOKS OF SCIENCE.

Philofophical Tranfactions of the Royal Society of London, for the Year 1801. *Part the Second. Quarto,* 214 *Pages, with Eighteen Plates.* London, fold by Elmfly.

THIS Part contains :—1. A Hiftorical and Anatomical Defcription of a doubtful Amphibious Animal of Germany, called by Laurenti, Proteus Anguinus. By Charles Schreibers, M. D. of Vienna.—2. Obfervations tending to inveftigate the Nature of the Sun, in order to find the Caufes or Symptoms of

Royal Society of London.

Royal Society of London.

of its variable Emiſſion of Light and Heat; with Remarks on the Uſe that may poſſibly be drawn from Solar Obſervations. By William Herſchel, LL. D. F. R. S.—3. Obſervations on the Structure and Mode of Growth of the Grinding Teeth of the Wild Boar, and Animal Incognitum. By Everard Home, Eſq. F. R. S.—4. Account of ſome Experiments on the Aſcent of the Sap in Trees. By Thomas Andrew Knight, Eſq.—5. Additional Obſervations tending to inveſtigate the Symptoms of the variable Emiſſion of the Light and Heat of the Sun; with Trials to ſet aſide Darkening Glaſſes by tranſmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made againſt ſome of the Arguments contained in the former Paper. By William Herſchel, LL. D. F. R. S.—6. On an improved Reflecting Circle. By Joſeph de Mendoza Rios, Eſq. F. R. S.—7. Obſervations and Experiments upon Dr. James's Powder; with a Method of preparing, in the Humid Way, a ſimilar Subſtance. By Richard Chenevix, Eſq. F. R. S. M. R. I. A.—8. Caſe of a young Gentleman, who recovered his Sight when ſeven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. James Ware, Surgeon.—9. An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. Humphry Davy, Lecturer on Chemiſtry in the Royal Inſtitution.—10. A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on Solar Light, when imbibed by Canton's Phoſphorus. By Nathaniel Hulme, M. D. F. R. S. & A. S.—11. Experiments on the Chemical Production and Agency of Electricity. By William Hyde Wollaſton, M. D. F. R. S.—12. Farther Obſervations on the Effects which take place from the Deſtruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafneſs. By Mr. Aſtley Cooper.

A Treatiſe

A Treatiſe on Aſtronomy, in which the Elements of the Science are deduced in a natural Order, from the Appearances of the Heavens, to an Obſerver on the Earth; demonſtrated on Mathematical Principles, and explained by an Application to the various Phenomena. By Olinthus Gregory, *Teacher of Mathematics, Cambridge.* 8vo. 522 Pages, with Nine Plates. London, ſold by Kearſley. Gregory's Aſtronomy.

Elements of Chemiſtry. By J. Murray, *Lecturer on Chemiſtry, Materia Medica, and Pharmacy.* 8vo. 2 vols. 692 pages. Edinburgh printed. Sold by Longman and Rees, London. Murray's Chemiſtry.

Obſervations on the Winds and Monſoons, illuſtrated with a Chart, and accompanied with Notes, Geographical and Meteorological. By James Capper, *formerly Colonel, and Comptroller-General of the Army and Fortification Accounts on the Coaſt of Coromandel.* Quarto. 234 Pages. Debrett. Capper on Winds and Monſoons.

Accounts of the three laſt mentioned Works will appear in our next.

Uber den Werth und Nutzen der Runkelruben.—On the Value and Uſe of the White Beet-Root. By J. G. W. 1801. 29 Pages. Glogau. J. G. W. on Beet-ſugar.

This pamphlet contains minute inſtructions for preparing ſugar, ſyrup, coffee, brandy, rum, and arrack, from the white beet-root (beta cicla). In order to obtain ſugar from the beet-root, the author directs the roots to be cut into ſlices, to expreſs the juice, to boil it down to a ſtrong conſiſtence, and to ſuffer it to cryſtallize. The reſidue, from which the juice has been expreſſed, may be uſed for obtaining brandy, but it is better to boil the beet-root to reduce it to a pulp, ſuffer it to ferment, and then ſubmit it to diſtillation. In order to deprive the ſpirit of the peculiar flavour of the root, it muſt be rectified over charcoal powder. Eighty pounds of beet-root yielded eight quarts of brandy. To obtain rum, the roots are likewiſe boiled, the juice expreſſed, and mixed with charcoal powder, evaporated to one-third, then ſuffered to foment, and afterwards diſtilled with the charcoal powder. In order to obtain arrack, the juice is to be evaporated to one-half, then ſuffered to ferment, and diſtilled.

Recherches

Des-Mortiers on Prussian Blue.

Recherches sur la Décoloration spontanée du Bleu de Prusse, &c.—Inquiries into the spontaneous Loss of Colour in Prussian Blue, and the spontaneous Return of its Colour; read at a private meeting of the Free Society of Sciences, Literature, and Arts, at Paris, by U. R. T. le Bowyer-des-Mortiers, *Member of the Society.* Paris. 8vo. 32 p. 1801.

A person had made for sale a pretty considerable quantity of blue paint, consisting of a mixture of white lead, Prussian blue, and nut oil. To prevent its drying he covered it with water some inches deep, and set it by till wanted. After a certain time, a person coming to buy some of the paint, he was much surprised to find it all white, except at the surface, where the colour was well preserved. He was preparing to add fresh Prussian blue to the paint, when, on grinding it in the open air, without any addition, he saw the colour return of itself, and on continuing this operation, it became as deep a blue as at first. He then covered it with oil, supposing it would keep better than under water; but he was disappointed, for the colour disappeared a second time throughout the whole mass.

The Society of Physic and the Arts at Nantes, to whom some of this paint was carried in its white state, spread part of it on writing paper, part on wood, and part on the wall of a window. After a longer or shorter time, the colour of the paint re-appeared with all its lustre, but that on the paper was most slowly restored.

What is the cause of this phenomena? Is it the oil, which, by undergoing a change, destroys the colour of the Prussian blue? Is it the air altogether, or one of its constituent principles, or any other substance mixed or dissolved in it, that restores the colour? These are the questions which the author endeavours to answer, in publishing the different interesting experiments he has made on the subject, from which he deduces the following conclusions.

1. The loss of colour in the paint is not owing to the decomposition of the oil, but to a change of surfaces, occasioned by the subsiding of the mass, and by the extinction of the luminous globules in the minute laminæ and in the pores of the colouring substance. 2. Neither the air altogether, nor one of its constituent principles, nor any thing it contains, is ne-

 cessary

ceffary to reftore the colour; for it revives *in vacuo* as well as in the open air. 3. Caloric, without the contact of light, is detrimental to the return of the colour, inftead of contributing to it, and even deftroys it. 4. Finally, the inteftine motion of its parts, however exerted, is fufficient to re-produce the colour more or lefs fpeedily, according as there are more or lefs light and motion.

Effai fur le Calorique.—An Effay on Caloric; or, Inquiries into the Phyfical and Chemical Caufes of the Phenomena exhibited by Bodies fubjected to the Action of the Igneous Fluid, with new Applications of them refpecting the Theory of Refpiration, Animal Heat, the Origin of Volcanic Fires, &c.; to which are added, An Effay on the Anomalies of Chemical Affinities; Experiments and Obfervations on Bell-metal; and a Defcription of the famous Alum Mine of Souvignuco, in Iftria, and of the Proceffes employed for extracting and purifying the native Alum: by Jofeph-Macy Socquet, M. D. *formerly Phyfician to the Army.* Paris, 8vo. 473 p. Price 5fr. 1801. Socquet on Caloric.

Natural philofophers are yet far from being agreed on the nature of caloric, and the phenomena afcribed to it. In this volume Dr. S. has publifhed eight effays on the fubject.

The 1ft treats of caloric confidered in its chemical and phyfical relations to other bodies, whence may be deduced the principal phenomena they exhibit when fubjected to the action of this fluid; as their capacity for caloric, their dilatation, fufion, gafification, tendency to equilibrium of temperature, &c.

In the 2d the author turns his attention to the caufe of the perpetual production of heat by the friction of bodies, and of the confequences which Count Rumford deduced from his own experiments on the fubject.

In the 3d he treats of the conducting power of liquids for caloric.

The 4th contains new views on refpiration, and the caufe of the production of heat in warm-blooded animals.

The fubject of the 5th is the nature of volcanic fires, the caufes of which the author deems independent of any local combuftion, produced by the decompofition of water or of any other oxide, or by fubmarine or fubterranean currents of air.

The

Socquet on Caloric.

The 6th contains feven cafes of chemical affinity:—1. Predifpofing affinities, as they are called: 2. The decompofition of fulphate of iron by cauftic ammoniac: 3. The decompofition of muriate of foda by fulphate of magnefia, or of the zero of Reaumur: 4. The reciprocal decompofition of alkaline phofphates by carbon, and of alkaline carbonates by phofphorus: 5. The decompofition of water in the folution of iron by diluted fulphuric acid; and the decompofition of the acid, not of the water, in diffolving quickfilver by the fame menftruum: 5. The precipitation of free fulphuric acid, by mixing the two fulphates of mercury and ammoniac: 7. Whence comes it, that thofe portions of folvents which are laft fixed in their bafes eafily feparate from them, while the firft portions, though of the fame nature, and fixed in the fame fubftances, adhere to them much more forcibly?

The author's refidence in Italy having given him an opportunity of travelling over almoft all the provinces of the quondam ftate of Venice, and fome other countries dependent on Auftria, he vifited a great number of productive and interefting manufactories. The extenfive alum mine of Souvignaco, in Iftria, of which there was no defcription extant, probably on account of its very remote and wild fituation, and perhaps becaufe it is of no ancient date, afforded him fome curious particulars refpecting the difpofition and nature of the ore, the fimple, ingenious, and partly new proceffes followed there, and the theory of the various phenomena they exhibit. Thefe are the fubjects of the 7th effay.

In the 8th Dr. S. gives a fummary of his experiments on the extraction of pure copper from bell-metal, the refults of which had already been publifhed in feveral Italian journals; together with fome other operations in the large way, which he had executed at Venice on different objects of art, particularly the feparation of foda from common falt*. The corrections, or rather additions, propofed by the Chev. Napione to the proceffes already known for decompofing the different kinds of bronze, were too interefting, for our author to omit making them known at large; particularly as he had an opportunity of convincing himfelf of their utility, by repeating the experiments of that learned mineralogift, at Turin.

* See *Opufcoli fcelti fulle Scienze e fulle Arti: da Carlo Amoretti: tom.* XX. Milan.

Atlas de la Partie Meridionale de l'Europe, &c.—An Atlas of the Southern Part of Europe, constructed to the Meridian of Paris, and consisting of forty Sheets, which, being on the same Scale, may be pasted together, so as to form the most comprehensive Map that has ever yet been published of the different States it includes: by C. Chanlaire, *Member of the Lyceum, and one of the Authors of the National Atlas of France.* Paris. 1801. **Chanlaire's Atlas of the South of Europe.**

This map is remarkable for the accuracy of its details, and may supply the place of a great many maps of single countries.

Mémoires d'Agriculture, &c.—Memoirs of Agriculture, and Rural and Domestic Economy, published by the Agricultural Society of the department of the Seine, and printed by order of the Prefect of the department. Vol. I. Paris. 8vo. 447 p. 1 plate. Price 4fr. **Memoirs of the Agricultural Society of the department of the Seine.**

An account of this work will appear in our next.

BOOKS OF SCIENCE,

Imported by Deboffe, of Gerrard-Street, Soho.

ANNUARE de la Republique Francoife, par le Bureau des Longitudes, pour l'An ix. Par. An. viii. in 18. br. 1s. 3d. **Books of science imported.**

Carnot de la Correlation des Figures de Geometrie. Par. 1801. 8vo. Fig. br. 7s.

Clairault Elém. d'Algebre. 6th Edit. 2 Vol. 8vo. Par. 1801. Fig. br. 14s.

Effais fur la Ligne Droite et les Courbes du Second Degré par Francois. 8vo. Par. 1801. 4s.

Gallitzin Recueil de Noms, &c. appropriés en Mineralogie fuivi d'un Tableau Mythologique. Brunfwick, 1801. 4to. br. 12s.

Haüy Traité de Mineralogie publié par le Confeil des Mines. Par. 1801. 4 Vol. avec Atlas. 2l. 2s.

Memoires

Books of science imported.

Memoires de l'Inſtitut National, 3e livraiſon An. ix. 3 Vol. 4to. Fig. cart. 3l. 3s.

Memoires des Societes ſavantes de la Republique Francoiſe, par Prony, Parmentier, Duhamel, &c. 4to. pour l'Année. 1l. 16s.

Monge, Traite Elementaire de Statiſtique Par. An. ix. 8vo. Fig. br. 6s.

Olivarius le Nord, Litteraire, Phyſique, Politique et Morale Ouvrage periodique publié tous les 3 Mois, les 16 Premiers Cahiers. 8vo. br. 2l.

O'Reily Eſſai ſur le Blanchiment. Par. 1801. 8vo. Fig. br. 7s. 6d.

Pallas Obſervations faites dans un Voyage enterpris dans les Gouvernemens Meridionaux de l'Empire de Ruſſie, pendant les Années 1793 & 1794. Leipz. 1801. Tom. 2. 4to. Atlas. 4l. 14s. 6d.

Philoſophie de Kaunt, ou Principes fondamentaux de la Philoſophie tranſcendentale, par Villiers. Metz. 801. 8vo. br. 6s.

Plans & Deſſins tirés de la Belle Architecture, ou Repreſentations d'Edifices executés on projettés, en 115 Planches par Stieglitz. Londr. 1801. Fol. Cart. 9l. 9s.

Principes d'Economie Politique: Ouvrage couronné par l'Inſtitut Nationale, par Canard. Par. 1801. 3s.

Traite Elementaire de Phyſique par Libes, Profeſſeur aux Ecoles Centrales de Paris. Par. 1801. 3 Vol. 8vo. Fig. br. 1l. 4s.

Voyage dans l'Inde et au Bengal, pendant les Années 1789 et 1790, par Grandpre. Par. 1801. 2 Vol. 8vo. Fig. br. 14s.

Voyage en Syrie, No. 20. Proof Plates, 2l. 12s. 6d.—Not Proof, 1l. 15s.

Herschel's observations on the Sun.

Fig. 1.

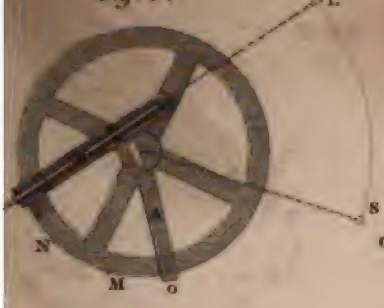
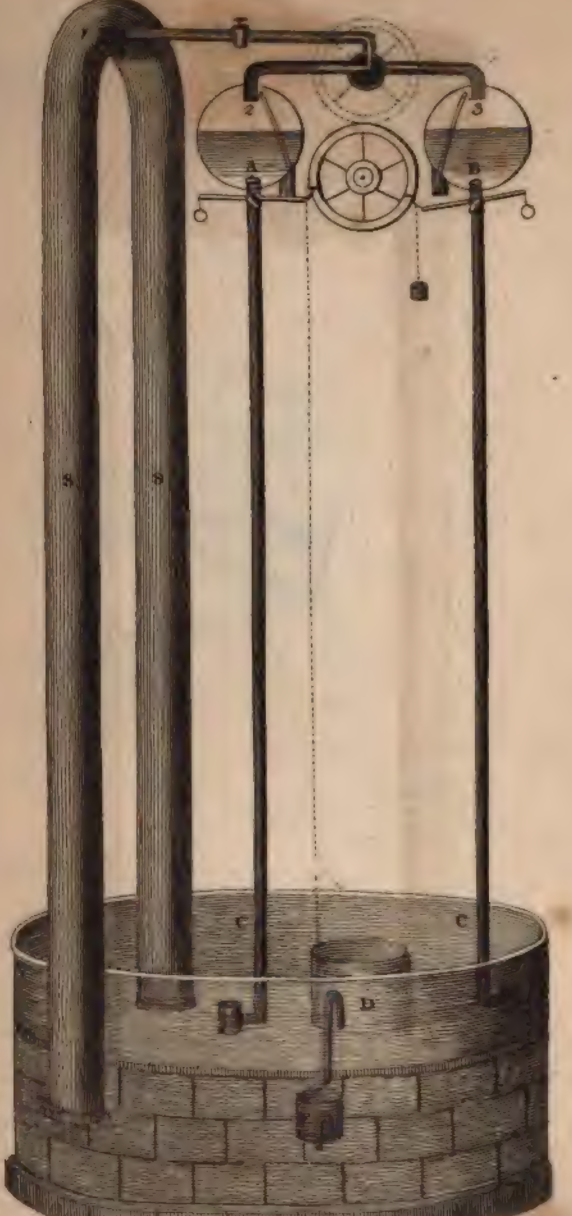


Fig. 2.

Mendoza's New Circular Instrument.

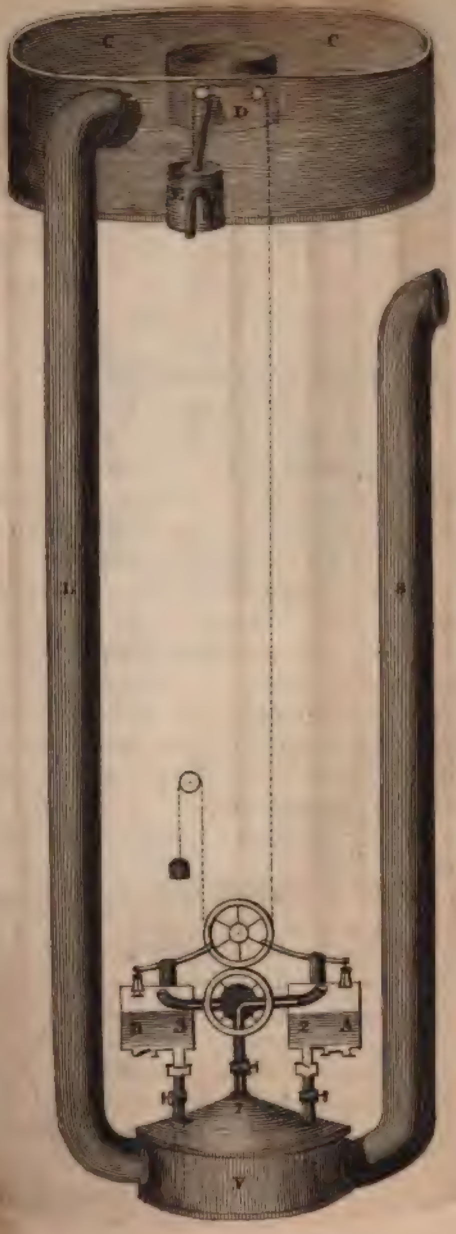
Close's hydraulic apparatus.

Fig. 1.



Close's hydraulic apparatus.

Fig. 2.



A

JOURNAL

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NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1802.

ARTICLE I.

On the suppofed Currents in hot Liquids. By THOMAS THOMSON, M. D. *Lecturer on Chemiftry in Edinburgh. (From the Author.)*

THERE is perhaps no fubject of enquiry in which we are more liable to deceive ourfelves than in the inveftigation of the internal motions of the particles of bodies. As thefe particles are individually too minute to come under the cognifance of our fenfes, their motions muft of courfe be invifible, and can only be inferred from certain vifible changes which we confider as the confequences, or at leaft as the conftant attendants of thefe motions. But unfortunately it feldom happens that the phenomena, from which we draw our inferences, are of fuch a nature as to entitle us to affirm that they can only originate from one fingle caufe, and from no other. They may be affigned in general to a variety of caufes; and much patient induftry, with no fmall fhare of addrefs, is neceffary to develope the true one; and even all the precautions which we can employ do not always prevent us from falling into error. The phenomenon, which forms the fubject of this paper, affords a very ftriking illuftration of the great caution with which we ought to draw conclufions, even when the inference feems at firft fight to follow irrefiftibly from the phenomena.

Great difficulty of inveftigating the internal motions of the particles of bodies.

Concife narrative of Count Rumford's experiments to render currents obfervable in fluids; by floating grains of amber.

Count Rumford was firft led to fufpect that fluids are non-conductors of caloric, by the appearance of certain opaque particles moving upwards and downwards in them, while they were cooling in a window, expofed to the direct light of the fun. He very naturally concluded that thefe motions were occafioned by the currents of the liquid moving in the fame direction with the opaque bodies. Hence he inferred that every individual particle of the liquid was cooled only by depofiting its caloric at the furface or the fides of the veffel, and that therefore thefe particles were incapable of giving out heat to each other. The contrivance by which he rendered thefe motions vifible to the naked eye was in the higheft degree fimple and ingenious. He mixed amber, in the ftate of fmall grains, with an alkaline folution, diluted with water till its fpecific gravity was exactly the fame with that of amber. The amber, of courfe, remained fufpended in any part of the liquid; but as foon as heat was applied, it began to move upwards and downwards, exhibiting the currents to the eye of the fpectator.

The motions of thefe grains have always been thought demonftrative of the exiftence of currents;

Nothing can appear a more fatisfactory proof of the exiftence of currents in the liquid than the motion of the amber; fo convincing is it indeed, that hitherto no perfon, as far as I know, has ever fufpected the poffibility of this motion originating from any other caufe. I have repeated the experiment frequently, and even exhibited it to others; and though the motions did not exactly correfpond with thofe indicated by Count Rumford, they never failed to produce the fulleft conviction of the exiftence of the very currents which that philofopher had indicated. Yet the fact is, that the motion of the amber is not occafioned by currents in the liquid, and very often exifts when there are really no currents at all.* This affertion will probably, at firft fight, appear rather extraordinary; but the proofs on which it is founded leave no room for doubt.

but they are not moved by currents.

Difficulties attending the fuppofition of currents as rapid as the motions of the amber.

When we reflect upon thefe fuppofed currents, feveral circumftances cannot fail to prefent themfelves, which do not admit of an eafy explanation. The motion of the amber is pretty rapid, yet the mere difference in the fpecific gravity of

* I neglect the trifling currents occafioned by the amber itfelf, which it would be extremely difficult to appreciate.

the

the hot and cold particles of the liquid, the causes assigned by Count Rumford for the existence of the supposed currents, is capable of producing only a motion so very slow, as scarcely to be visible. But what is still more puzzling, if possible, is the total discordance between these rapid currents and the slow rate at which the liquid cools. If the water actually moved with the rapidity indicated by the amber, and if it parted with the tenth part of the caloric necessary to produce that rapidity, the whole of the liquid would cool down to the temperature of the atmosphere almost in an instant: whereas, in fact, several hours are always necessary to produce that effect.

Experiment 1. Upon water, tinged blue, was poured clear water with such care as not to mix them. Several opaque bodies floated in these fluids.

These apparent anomalies suggested the possibility of the motion of the amber being occasioned by some other cause than currents in the liquid, and made it necessary to examine the matter with more attention. The following experiments occurred to me as the simplest method of determining whether the supposed currents existed or not. Into the glass vessel A, Pl. VIII. Fig. 2, I poured water, tinged blue with the juice of red cabbage, till it stood at the height *m*. Then by means of a slip of cork swimming on the surface of the blue infusion, and a funnel terminating in a long capillary tube, I succeeded (after several unsuccessful attempts) in filling the vessel with water to the height *n*, without mixing it with the blue infusion. Thus I had two liquids in the vessel A, both very nearly of the same specific gravity, the undermost of a blue colour, the uppermost colourless, and the plane *m*, which separated them, pretty well defined. There were several opaque bodies floating through the blue infusion, which answered the purpose of the amber.

It was expected, when heat should be applied, *either that* the lower fluid would ascend *in a blue current* and carry up the opaque bodies, *or that* these bodies would ascend alone through both fluids, and consequently *without the mediation of any* current.

My intention was to apply heat to the bottom of the vessel A, which I knew would produce the supposed currents, that is to say, would set the floating bodies in motion. Now if these floating bodies were really put in motion by currents, it is evident that part of the blue infusion must accompany them. But this would become evident whenever they passed the plane *m*, and entered into the colourless medium: for in that case they would be accompanied by a blue current, which would be visible to the eye. Whereas if the floating bodies were in motion while the water continued at rest, they would pass the plane *m*, and move through the colourless water

without

without carrying any of the blue infufion along with them. I accordingly lighted a lamp, and put it at fome diftance below the veffel A, placing the veffel exactly between my eye and the window. The lamp was at fuch a diftance, that it was fome time before the floating bodies began to move. At laft, however, they afcended gradually, paffed the plane *m*, and entered the colourlefs medium, *without being accompanied by any of the blue infufion.* They then defcended again in the ufual manner; and this alternate motion continued for fome time, without occafioning a mixture of the two liquids. Thus I obtained a full demonftration that the motion of the floating bodies was not occafioned by currents in the liquid, but was altogether independent of any fuch currents. Every time that a floating body paffed the line *m*, the blue infufion was agitated, and its furface raifed into a wave. By the conftant repetition of this undulation, the plane *m* became ill-defined, and gradually afcended higher and higher, till at laft almoft the whole of the water acquired a blue tinge. But it was almoft ten minutes before this happened, and the motion of the floating particles, which continued without interruption during the whole time, was altogether unequivocal.

On trial the bodies rofe and circulated through both fluids without any indication of a current; and ten minutes elapfed before the motion had carried the blue tinge through the upper fluid.

Experiment 2: An alkaline folution, with floating grains of amber, was boiled, and left to cool to 150°. The upper part, to the depth of half an inch, was then tinged. No change whatever took place in the tinged fluid, though the amber circulated through the whole fluid, while cooling.

It was ftill neceffary to repeat the experiment with the amber itfelf, in order to afcertain whether or not its motions were of the fame nature with that of the floating bodies in the laft experiment. I therefore filled the glafs veffel A, to the height *n*, with an alkaline folution containing amber floating in it. I then heated the folution boiling hot by holding the veffel over a fire, and even allowed it to boil for fome time. The veffel was then fufpended in a room, where the temperature of the air was 50°, and the alkaline folution allowed to cool down to 150°.* I then tinged the furface of the folution by means of a drop or two of the infufion of red cabbage, fo as to form a coloured ftratum of about half an inch thick at the furface of the folution.† The cooling continued without interruption, and the liquid reached the temperature of the air without the fmalleft alteration in the coloured ftratum, or any mixture of it with the colourlefs

* By previous trials I had afcertained that at a higher temperature the experiment does not fucceed.

† The colour thus communicated is yellow when only a little cabbage juice is ufed; too much gives a green colour.

folution

ſolution below. Yet, during the firſt part of the cooling, the amber was moving upwards and downwards; particles of it often come to the very ſurface, and deſcended again to the bottom. This experiment proves deciſively that the motion of the amber is not occaſioned by currents, and that there were, in reality, no currents at all during the whole time of cooling from 150° to 50°.

Why does the amber thus move *through the fluid?*

Having thus proved that the motion of the amber is not owing to currents, but that it forces its way through the liquid; it becomes a point of ſome conſequence to aſcertain, if poſſible, the cauſe of this motion. Perhaps the following obſervations will contribute to throw ſome light on the ſubject.

Obſervation 1. Amber expands leſs by boiling heat than the alkaline ſolution.

1. When an alkaline ſolution containing amber, as exactly as poſſible of the ſame ſpecific gravity with itſelf, is heated in a glaſs veſſel nearly to the boiling temperature, and then ſuſpended in the air to cool, the whole of the amber ſinks to the bottom. Hence we ſee that, though the amber is of the ſame ſpecific gravity with the cold ſolution, it is ſpecifically heavier than the ſame ſolution when hot. Conſequently amber does not expand ſo much when heated as the alkaline ſolution does.

Obs. 2. Each particle of amber is carried up by an adherent air-bubble, which it loſes at the ſurface, and then falls again.

2. But the amber does not remain long at the bottom; the grains of it gradually riſe one after another, and aſcend to the ſurface, but with very different velocities, ſome rapidly, others ſlowly. After touching the ſurface they always fall down again to the bottom. Theſe alternate motions continue for ſome time; but the number of aſcending and deſcending particles gradually diminiſhes, and at laſt they all ſettle at the bottom. It is no uncommon thing for two particles of amber, one aſcending, the other deſcending, to meet together in ſome part of the ſolution. In that caſe they ſometimes exchange motions; the one that was formerly aſcending now ſinking to the bottom, while the deſcending particle returns again to the ſurface. If the experiment be made with a tranſparent ſolution, and in a good light, every aſcending particle of amber will be obſerved to have an air bubble attached to it which buoys it up. This bubble ſeparates at the ſurface of the liquid, and the amber, thus deprived of its buoy, tumbles down again to the bottom. When two particles meet, the air bubble ſometimes paſſes from the one to the

the other, which occafions their reciprocal change of motion' Thefe air bubbles are of different fizes, hence the different velocities of the pieces of amber.

Obs. 3. The amber acquires a higher temperature by boiling than the folution itfelf, and confequently it generates fteam.

3. If the alkaline folution be made to boil for a few minutes in a glafs veffel, and then fufpended immediately in the air to cool, bubbles of fteam continue for fome time to detach themfelves in great abundance from the pieces of amber, and occafion a very brifk motion in thefe pieces. Hence we fee that the amber acquires a temperature higher than 212°, and that it gradually parts with this excefs of caloric to the furrounding liquid.

Obs. 4. Irregular motions after the fteam has ceafed; followed by fubfidence.

4. After all extrication of fteam ceafes, the amber continues to move rapidly upwards and downwards in a very irregular manner. The rapidity of thefe motions gradually diminifhes, and when the folution has cooled down to about 140° (the air being 50°) the amber fettles at the bottom.* Thefe motions are too rapid to be occafioned by the cooling of the liquid, yet they are certainly connected with its high temperature. I would have fuppofed that they indicated the currents into which the liquid was thrown by boiling, as they are extremely fimilar to what takes place when the liquid is agitated violently, were it not that they laft much longer, and that they always ftop when the difference between the temperature of the liquid and of the air is about 90°.

Obs. 5. Afcent of the amber; whence it is inferred that it cools lefs rapidly than the fluid.

5. After the amber fettles at the bottom, the cooling goes on without any thing remarkable till the liquid reaches the temperature of about 100° (the air being at 50°). At that temperature fome of the amber may be obferved fufpended juft above the bottom of the veffel; and, as the cooling advances, the amber gradually rifes higher and higher. No motion can indeed be perceived, but if the fituation of any individual particle be obferved at intervals, it will appear evident that it has changed its place, and afcended nearer the furface.

* This experiment requires that the amber be in the ftate of a pretty fine fand, otherwife it fettles at the bottom at the very firft. But even then the currents may be diftinguifhed by the naked eye, if the glafs veffel be tranfparent. This fact is curious, and can only be explained by fuppofing a greater interval than ufual between the currents and the reft of the water, or by fuppofing their fpecific gravity to be different.

surface. By the time the liquid has reached the temperature of the air, some of the amber may be observed floating on its surface, some of it a little below the surface, and, in short, in almost every part of the liquid. After this the liquid may be left at rest as long as you please; but the amber does not change its situation, unless some very considerable change takes place in the temperature of the surrounding air. Here we see that the water cools down more rapidly than the amber; so that at a certain period the amber becomes specifically lighter than the water, and therefore rises; extremely slowly indeed, because the difference between the specific gravity of the two bodies is small. I supposed at first that the rise of the amber was owing to the descent of the superior strata of the liquid, which, of course, would have forced up the strata in which the amber was; but I was mistaken. For when the surface of the solution was tinged yellow, the yellow stratum remained neatly defined, and without changing its place during the whole of the process.

This ascent i[s] not caused b[y] current.

Annotation. Atwood, in his Treatise on Motion, Sect. V. Prop. 12, demonstrates that a sphere of evanescent weight will ascend in any dense fluid uniformly with a velocity equal to what it would have acquired by falling in free space through four third parts of its diameter. This is the greatest possible velocity of ascent of a sphere in any fluid. It may seem absurd to attempt a calculation, one of the assumptions of which shall be the diameter of an integrant particle; but if we take any gross quantity (such, for example, as the millioneth part of an inch), for the diameter of a particle of water at its utmost expansion by heat, and deduce its velocity of ascent (which in that case could scarce amount to the two hundredth part of an inch in a minute), we shall find little reason to admit that ascending currents can be produced by the mere enlargement of the particles heated only by contact with the containing vessel. This, however, must necessarily be supposed if the particles be non-conductors with regard to each other.

Ascent of a sphere in a fl[uid]

Motion of t[he] particles of w[ater] by mere exp[an]sion must be [ex]tremely slow

The excellent paper before us directs our attention to the mechanical cause of that circulation, which undoubtedly takes place during the communication of heat to fluids from below, and

Circulation [of] fluids by ch[ange] of temperat[ure]

and is the principal reafon of their much fpeedier augmentation of temperature in this way. The extrication and afcent of elaftic fluid appears, in moft and probably in all cafes, to precede, and at laft to produce circulation in denfe fluids. And on this object there is ftill an extenfive field for refearch, concerning the manner in which heat is communicated through fluids. But it would be by no means fair to anticipate, by crude conjecture, thofe explanations which we may expect to obtain from the experimental refearch of others. W. N.

II.

A Memoir on the Art of making Gun-Flints. By CITIZEN DOLOMIEU. *

No information to be found in books on the art of making gun-flints.

THE art of making gun-flints, which has been long practifed in a fmall territory fituated between the two neighbouring departments of Loir-Cher and L'Indre, is directed to an object, of which the whole value, commercially confidered, is of little magnitude; but which is of the firft neceffity in the ufe of fire-arms of every defcription. The author of the prefent Memoir was defirous of acquiring fome information refpecting it, but found that it was to no purpofe that he extended his refearches to a variety of works on mineralogy and different fubjects of the arts and trade. The Encyclopédie is filent on this procefs; excepting in the repetition of a ridiculous prejudice concerning the re-production of flints in places whence they were excavated. From the precifion with which the figure is given to thefe ftones, it has even been fufpected that they could hardly be afforded at the low price they bear, if in its firft ftate the material were not even foft. But the art is extremely fimple in its procefs; it requires a very fmall number of inftruments, a fhort apprenticefhip, and a very moderate degree of fkill to form, by mere fracture, figures fo accurate, faces fo fmooth, outlines fo direct, and angles fo neat, that the ftone feems as if cut on the wheel of the lapidary. Five or fix fmall blows of the hammer, during one minute of time, are fufficient to produce the

* From Memoires de l'Inftitute Nationale, III. 348, in part abridged.

the same perfection of figure as would require more than an hour's labour, if the sections were to be made by grinding against harder substances, or friction with emery. Less than a farthing will pay for a gun-flint from the hand of the workman, but fifty times that sum would be insufficient for its purchase if it were fashioned by any other process.

The author regularly proceeds to examine first the material best adapted to the use in question, the instruments employed, and the manipulation by which the stones are fashioned. Order of description.

With regard to the material, every kind of stone, capable of producing strong sparks when struck against steel, may be used as a gun-flint, if it can but be fashioned by simple and cheap means. But even in this case there are some motives of preference; such, for example, as that the scintillation should be produced with the least possible blow, and with no considerable wear or abration of the steel. These reasons of predilection are in favour of the siliceous stones, when compared with those which are called quartzose. But the silex or flint, properly so called, possesses not only this kind of superiority, but another property, that it is more partioularly susceptible of being broken into fragments or plates, which require but very little labour to give them the requisite form and dimensions. The material or stone most fit.

Among the silex it is therefore that the fabrication of gun-flints have found the material truly proper for the exercise of the art. And among the numerous varieties of this species of stone, there is only one which can be advantageously fashioned by the hammer alone. The agates and chalcedonies, which are also applied to this use, are brought to the requisite form by the mill of the lapidary. The makers of gun-flints in France denominate the stone of which they make use *caillou*, and they themselves are called *caillouteurs*. The word *caillou* is used by them to denote the best and most serviceable kind of flint; whereas, in the other parts of France, it denotes a pebble; that is to say, a rounded stone, whatever may be its nature. Silex or flint preferred to quartz, because easily fashioned.

The flint of the workmen in gun-flints belongs to that species of silex which naturalists have denominated silex gregarius, silex ignarius, or the feuerstein of the Germans, &c. But every coarse flint is not proper for this use. In fact, the best stone is far from being plentiful in nature. Many countries The true *silex ignarius* is scarce;

tries are entirely deprived of them; and the author thinks that it may probably be affirmed, that France almoſt alone poſſeſſes that variety of ſilex which can be eaſily broken into gun-flints, ſince he cannot ſuppoſe that the art of making them could remain a myſtery to other nations who do not practice it, though they make great uſe of the flints: the art itſelf being ſo ſimple, that they muſt have ſpeedily acquired it, if they have poſſeſſed the material.

Denomination. In his deſcription of the variety of ſilex here alluded to, he gives it the name of ſilex pyromachus to expreſs its uſe, which he prefers to the term ſilex ſclopetarius, as being more muſical.

The external characters are:

External characters. The ſilex pyromachus, when dug up, is always covered with a white external cruſt, one or two lines or more in thickneſs, of an earthy, chalky appearance, and looſe texture, much ſofter and leſs heavy than the ſilex it envelopes. The external form of the maſſes of good ſtones of this deſcription has a ſomewhat convex ſurface, approaching to the globular figure. Thoſe of irregular forms are full of imperfections. The beſt ſtones are not very large. They ſeldom exceed the weight of twenty pounds, and they ought not to be of leſs weight than one or two pounds. Their aſpect, when broken, is greaſy, ſhining a little, and the grain is ſo fine, that it is imperceptible. The colour of theſe good ſtones may vary from the yellow colour of honey to a blackiſh brown. In this reſpect it is to be noted that the value of a ſtone does not depend on its colour, but on the uniformity of the tint, which becomes leſs intenſe when the ſtone is reduced into thin ſplinters. The flints of the two departments firſt mentioned are yellowiſh. Thoſe of the chalk hills on the banks of the Seine are blackiſh brown. Both the one and the other, when pulveriſed, are perfectly white. The ſilex pyromachus ought to poſſeſs an uniform ſemi-tranſparence, of a greaſy aſpect, to ſuch a degree, as to admit letters to be diſtinguiſhed through a piece of the ſtone of one-fiftieth of an inch thick, laid cloſe upon the paper. Its fracture muſt be ſmooth and equal throughout, and very ſlightly conchoidal; that is to ſay, convex or concave. This kind of fracture is one of the moſt eſſential properties upon which the faculty of being divided into gun-flints depends,

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The workmen select the stones proper for their use by their external character. They compare that part of the masses of silex, which is uniformly semi-transparent and coloured, to the inner skin of bacon, which they call *couenne*. They say that one flint has more or less *couenne*, or is more or less fat than another. They assert that the upper part of a flint is always of a better quality than the lower. Flints are considered as imperfect or intractable when naturally deprived of any of the external characters before indicated, or when injured by long exposure to the air. Most of the masses are subject to have whitish opaque spots and a kind of knots where the material is harder than in the other part of the stone. When these accidents are too abundant, the stone is rejected as useless.

Selection of the best flints from their greasy aspect.

The physical characters are as follow:—Specific gravity of the white silex pyromachus from the banks of the Cher proved to be 26041; that of the blackish kind from the chalk isles of Laroche Guion was 25954. In this respect it does not differ essentially from the other varieties of silex, of which the specific gravities are usually between 26100 and 25900. Hardness a little superior to that of jasper, but inferior to that of agate and chalcedony. It is nearly the same as that of the other common flints. Brittleness. It is more brittle than most other siliceous stones. The light coloured is more so than the darker; these last being rather more scintillant, and and wear away the hammer more quickly. When two pieces of the silex pyromachus are strongly rubbed together, they emit the peculiar well known smell of siliceous stones, but in this variety it is more strong than in any other.

Physical characters. Specific gravity, hardness, brittleness, &c.

Chemical characters. Action of the air.

The silex pyromachus, deprived of its natural coating, and exposed for a long time to the changes of the atmosphere, acquires a second white friable coating, consisting of the silex reduced to powder; and even its internal part loses its greasy appearance and semi-transparency, so as to become whitish. In this case the specific gravity, which was 25954, becomes 25754, consequently it has lost 200 parts of the weight it possessed at first.

Chemical characters. Action of air.

The silex pyromachus is sometimes too moist when taken out of the quarry. It requires to be dried; but if by too long exposure to the air or the wind it loses a certain portion of humidity

It contains moisture.

humidity which is often very perceptible when it is recent, it can then no longer be broken into gun-flints, as its fracture is less easy. The workmen carefully reject all those which have lost this favorable degree of moisture. Perhaps they might be restored by keeping them in a damp place, or covering them with earth, and by these means at least they might succeed in preserving those intended to be worked up in winter.

Effect of heat. Oxidation.

When the fragments are thrown upon a red hot plate of iron it flies and cracks, and becomes opaque. When projected in powder upon nitre in fusion, it gives a few sparks with slight inflammation and detonation.

When calcined in a test it loses one 250th part of its weight, increases in bulk, becomes extremely white, and so brittle as to be almost friable. In this state it resembles the finest porcelain biscuit.

Distillation.

When distilled in a retort by strong heat it affords a little carbonic acid gas, and a quantity of water amounting to 200 parts of the weight before indicated as its specific gravity; but gives no sign of the combustible matter which in the preceding experiment caused the nitre to detonate.

The flint is an imperfect hydrophanes.

This water, which appears essential to all the flints, and may be called their radical water, is the cause of their transparency. Exposure to air by drying them renders them opaque; so that they may be considered as imperfect hydrophanes; for they do not again absorb, but with difficulty the water necessary to their transparence. This water also contributes to the connection of their integrant particles, whence their fracture becomes more equal, and is harsh when they have lost it. These flints when recently dug up even afford an aqueous vapour when struck, and the face of the fracture is humid, and as it were moist.

Chemical analysis.

Chemical analysis; fusion with potash; precipitation of the silex, &c.

Citizen Vauquelin examined 100 parts of silex pyromachus of a brownish colour, and uniformly semi-transparent from the hills of La Roche Guion. He mixed the mass with 400 grains of very pure potash, which by fusion in a silver crucible afforded a compound, which after cooling was diffused in water, and then super-saturated with muriatic acid. The very clear solution was evaporated to dryness, re-dissolved in water, and the silex thus saturated, and left upon the filter after being well washed,

washed, dried and ignited, weighed 97 grains. Ammonia was afterwards added to the clear liquid, where it produced a slight yellowish white precipitate, which after being well washed and dried weighed one grain, and was found to be a mixture of alumine and oxide of iron. The fluid separated from this small portion of iron and alumine, gave no other precipitate on the addition of carbonate of potash, and the waters used in the washing left no residue when they were evaporated to dryness. The result therefore was silex = 97 grains, alumine and oxide of iron = 1, loss = 2. The author considers it to be a very remarkable fact, that the silex pyromachus should contain only silex and water, the alumine and iron being too small in quantity to be considered as essential to its composition, or to influence its habitudes. Quartz also, from the analyses which have been made, appears to contain only silex; yet the more he examines the two substances, the more he finds reason to suppose them essentially different from each other; since the one refuses to assume the crystalline form, and the other assumes it and becomes clear, even in contact with the flint itself. The one appears to be incapable of admitting water into its composition, while the other constantly contains it until it begins to be decomposed. He offers a query, whether the difference may not consist in the small portion of combustible or fatty matter, which caused the detonation with nitre, or whether the quartz may not, like alum, acquire its property of crystallizing from the addition of some other substance. This question, as he remarks, must be resolved by future experimental enquiries. Difference between silex and quartz.

The analysis of the whitish spots afforded silex 98 grains, oxide of iron 1, carbonate of lime 2. That of the absolutely opaque parts gave five parts more of carbonate of lime; and lastly, the analysis of the white coating naturally enveloping the masses, afforded 86 parts of silex, 1 oxide of iron, 10 carbonate of lime, and 3 loss. Those analyses which afforded no alumine, shew that this earth is not essential to the silex, and the absence of lime in the first analysis shews that it was an accidental ingredient in the latter.

Mineralogical situation. In France in the environs of St. Aignan, situated in the department of Loir-Cher, and in that of L'Indre, and the departments which occupy the vallies of Siene and Marne, are principally the places where this stone Mineralogical situation.

ftone is found. It exifts in the chalky calcareous ftones, in chalks more or lefs fine and folid, and in marles. They form horizontal ftrata, by the manner in which the large and fmall maffes are placed befide each other. Neverthelefs, as the blocks of filex do not accurately touch each other, there is no folution of continuity between the upper and lower maffes of chalk.

Out of twenty beds of filex lying one above the other, at a diftance of twenty feet or lefs, there will frequently be no more than one, and very feldom two, which afford good ftones of this defcription; but in the bed which affords them, almoft all the blocks have a greafy appearance, and in the other ftrata fcarcely any of this defcription will be found. Accordingly the good ftrata are followed by fubterraneous excavations, frequently at confiderable expence, while the others are neglected.

On the banks of the Cher the flints are explored in a plain, by digging fhafts to the depth of 40 or 50 feet, from whence horizontal galleries are carried into the only good ftratum which is known.

On the banks of the Seine in the hills of La Roche Guion, the cliffs of chalk prefent fteep precipices, where the ftrata of filex are expofed; and one of thefe ftrata, which contains good ftones for gun-flints, is about fix toifes from the upper furface of the great mafs of chalk.

Inftruments.

Defcription of tools or inftruments.

The inftruments ufed for fafhioning the gun-flints are four in number:

The mace.

1. A fmall piece of iron or mace, with a fquare head, Plate V. Fig. 1. the weight of which does not exceed two pounds, or perhaps a pound and a half, with a handle feven or eight inches long. This inftrument is not made of fteel, becaufe if it were too hard, its ftroke might fhatter the flint, inftead of breaking it by a clear fracture.

The hammer.

2. A hammer with two points, in which the pofition of the points is of confequence as to the nature of the ftroke, Fig, 2. This hammer, which muft be of good fteel well hardened, and does not weigh more than fixteen ounces; fome do not exceed ten. It is fixed on a handle feven inches long, which paffes through it in fuch a manner, that the points of the hammer are nearer the hand of the workman, than the center of

gravity

gravity of the mafs. The form and fize of the hammers of different workmen vary a little, but this difpofition of the points is common to them all, and is of confequence to the force and certainty of the blow.

3. A little inftrumen tnamed Roulette (roller) which reprefents a folid wheel, or fegment of a cylinder, two inches and one third in diameter. Its weight does not exceed twelve ounces, it is made of fteel not hardened, and is fixed on a fmall handle fix inches long, which paffes through a fquare hole in its center. Roulette.

4. A chiffel bevelled on both fides, feven or eight inches long, and two inches wide, of fteel not hardened; it is fet on the block of wood which ferves as a work bench, out of which it rifes to the height of four or five inches. To thefe four inftruments we may add a file, for the purpofe of reftoring the edge of the chiffel from time to time. Chiffel.

The procefs:

After felecting a good mafs of filex, the whole operation may be divided into four manipulations. Manipulations.

1. To break the block. The workman being feated on the ground, places the flint on his left thigh, and ftrikes it gently with the larger hammer, Fig. 1. to divide it into portions according to its fize, that is to fay, of about a pound and a half each, with broad furfaces nearly flat. He is careful not to crack or produce fhakes in the flint by ftriking it too hard. Break the block.

2. To cleave the flint, or break it into fcales. The principal operation of this art is to cleave the flint well: that is to fay, to feparate from it pieces of the length, thicknefs, and figure, adapted to be afterwards fafhioned into gun-flints; and in this part the greateft degree of addrefs, and certainty of manipulation are required. The ftone has no particular direction in which it can be moft eafily broken. The courfe of its fracture depends entirely upon the choice of the workman. In this procefs he holds a piece of flint in his left hand, not fupported, and ftrikes with the hammer, Fig. 2. on the broad faces produced by the firft fracture, in fuch a manner as to chip off the white coating of the ftone in fmall fcales, and to lay bare the filex in the manner reprefented, Fig. 5.; after which he continues to ftrike off other fimilar portions of the pure filex. Thefe pieces are nearly an inch and a half wide, two inches and a half long, and one fixth of an inch thick in the middle. Cleave or fcale the flint. No particular direction of fracture.

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Figure of the scales.

They are flightly convex within, and confequently leave a fpace fomewhat concave, terminating longitudinally in two lines, fomewhat projecting, and nearly ftrait. The prominent edges produced by the fracture of the firft fcales, muft afterwards conftitute nearly the middle of the fubfequent pieces; and thofe pieces only, in which they are found, can be ufed to form gun-flints.

In this manner the operator continues to cleave, or fcale the ftone in different directions, until the natural defects of the mafs render it impoffible to make the fractures required, or until the piece is reduced too much to receive the fmall blows which feparate the pieces.

3. To fafhion the flint.

Fafhion or form the flint.

The gun-flint, Fig. 7. may be diftinguifhed into five parts, namely, 1. the edge, or bevel part, which ftrikes the hammer or fteel. This is two or three twelfths of an inch in width. If it were broader it would be too liable to break, and if more obtufe it would not afford a brifk fire. 2dly. The fide edges, which are always fomewhat irregular. 3dly. The back edge, moft remote from the hammer where the ftone poffeffes its intire thicknefs. 4thly. The under furface, which is fmooth and flightly convex. And 5thly. The upper face, which is flightly concave, and receives the action of the upper claw of the cock, in which it is fixed for fervice.

Operation.

In order to fafhion the ftone, thofe fcales or chips are felected, which have at leaft one longitudinal prominent angle. One of the two edges is fixed on to form the ftriking edge; after which the two fides of the ftone which are to form the fide edges, and that which is to form the hinder edge are fucceffively placed with the convex furface upon the edge of the chiffel, which is fupported with the fore-finger of the left hand, at the fame time that a fmall blow or two is given above the point of fupport with the Roulette, Fig. 3. by which the ftone breaks exactly along the edge of the chiffel, as if it had been cut. In this manner the fides and pofterior edge of the ftone are made.

Complete the edge

4. The ftone being thus reduced to its proper figure, the finifhing operation confifts in completing its edge in a ftrait line. For this purpofe the ftone is turned, and the under flat part of the edge is placed on the chiffel, in which fituation it is completed by five or fix fmall ftrokes with the Roulette.

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The whole operation of fashioning a gun-flint is performed in less than one minute.

A good workman can prepare a thousand good chips or scales in a day, if his flints be of good quality, and he can also fashion five hundred gun-flints in a day; consequently in three days he will cleave and finish a thousand gun-flints without further assistance. The rate of working. On thousand flints in three days.

This manufacture leaves a great quantity of refuse; that is to say, about three fourths of the whole stone: For there are not more than half the scales which prove to be well figured, and nearly half the mass in the best flints is incapable of being chipped out: so that it seldom happens that the largest piece will afford more than fifty gun-flints. The larger pieces of refuse are sold for the culinary purpose of striking a light. Much refuse or waste.

The gun-flints when completed are sorted out, and sold at different prices, according to their degrees of perfection, from 4 to 6 decimes (or pence) the hundred. They are classed into fine flints and common flints; and according to their application into flints for pistols, fowling pieces, and muskets.

The fabrication and commerce of gun-flints in France is in some measure confined to three communes of the department of Loir-et-Cher, and one department of the Indre, as was before mentioned, namely, the commune of Noyers, 2,400 metres east north-east of St. Aignan; the commune of Couffy at 5,600 metres, and that of Meunes at one miriameter east south-east, and in the latter department, the commune of Lye 9 kilometers to the south-west of St. Aignan. The inhabitants of these communes employed in this species of industry are about 800, and they have excavated great part of the plain they inhabit. Local situation of the manufactories in France.

A single workman named Stephen Buffet, who emigrated from the commune of Meunes to the banks of the Seine, where he has carried on this art for about thirty years by himself, was the person from whom Dolomieu obtained the present instructions. There are a few other places in France where this art is practised, but in none to the extent of the places before mentioned. The author has not met with this manufactory in any other countries, except in the territory of Vicenza, and one of the cantons of Sicily. He remarks that it may be carried on elsewhere, though probably overlooked by travellers, on account of its apparent insignificance. I believe it is practised at Purfleet, in the county of Kent, and in various other parts of England. Elsewhere practised.

 III. *Letter*

III.

Letter from Dr. BEDDOES *on Prognostics of the Weather, the Effects of the Nitrous Oxide, and other Objects.*

To Mr. NICHOLSON.

SIR,

Clifton, Jan. 9, 1802.

Probability that the ensuing spring and summer may prove cold.

SINCE the late fall of snow, I have been asked by several people, how I would now apply the principle, stated in a letter, inserted in one of your former Numbers *. My answer was, that it induced me to suspect a corresponding great fall in the N. and N. E. regions. If this be the fact, and if no unknown calorific process take place between us and the equator, it would furnish an unfavourable prognostic with regard to the weather of the ensuing spring and summer.

How the weather is to be foreknown.

We can, I suspect, only attain a degree of prescience concerning the seasons, by comparing the order of the most obvious meteorological events. This, you know, was the way in which the first improvements were made in astronomy. In order to gain another point of comparison, I beg leave diffidently to propose a conjecture, respecting the duration of different falls of snow. The duration, I suppose, will bear some proportion to the quantity on the ground. For example, *if an inch of snow fall, when the thermometer has been within the twelve preceding hours above 32° it will either lie a much shorter time than if six inches fall, so as to lie under the same condition.* Perhaps the duration will generally be at least equal to the squares of the depths.

Conjecture.

Atmospheric currents of different temperature.

You will suppose that I found my conjecture upon some sort of reasoning. I deduced falls both of snow and rain, after Dr. Hutton, from the mixture of airs, unequally heated. I consider snow falling and lying in quantity, as the sign of a great current of air from the polar regions, particularly when the wind has blown for some time from the opposite points;

* Philos. Journal, Quarto V. 131. The principle is that, *If there be an unusual fall of snow in the countries on the N. and N. E. our summer, cæteris paribus, will be cold and wet.*

that

that is, when a large body of air, at above 32°, has gone to the northward of thefe iflands. If the fnow lie to any depth, I imagine this to be a fign of the preponderance of an under-current from the fame quarter, which will generally produce a froft of fome continuance, as well as fnow in the firft inftance. Where a S. or S. W. wind prevails, the fnow will change into rain, and not lie to any depth.

Query.

I abridge this precarious fpeculation, (which it would be eafy to extend into a pamphlet) and fupprefs many explanations, in order merely to propofe, as a query, *Whether a fnow, like that which now covers the earth, be not always actually the forerunner of a froft of fome days, and generally of fome weeks, continuance?*

On the intermediate thaw.

During the prefent week we had the curious phenomenon of a thaw for (I believe) near 48 hours, with the wind at N. E. which no doubt arofe from a confiderable upper current from the contrary quarter, at no great elevation. But the quantity of fnow, already on the ground, made me venture repeatedly to fay, that the weather would not foon change to a compleat thaw.

Medical effects of the nitrous oxide, &c.

In a fhort time I intend to put to prefs a report of the operations at the pneumatic inftitution here. The nitrous oxide has juftified the expectation, originally formed of its powers in palfy, and alfo as a *reftorative*, if I may employ this abufed word, to certain conftitutions uniformly broken down, and has proved an admirable *foother* of the fufferings of old age. We (I fpeak of myfelf and other perfons, concerned at the inftitution) have frequently witneffed in new patients the temporary reftitution of the voluntary power over a perfectly paralytic limb during the act of inhalation. This power has become permanent afterwards in fome inftances, but not always, though perhaps not from want of efficacy in the air. It has alfo been difcovered here, that oxigen gas, long inhaled, affects the growth and the internal parts, efpecially the bones, in a very extraordinary manner. The effect on the bones I had anticipated, and the altered growth, feems a natural confequence. Some accurate drawings by Mr. King will ferve to exhibit thefe effects.

'We ſhall have to report alſo ſome conſiderable improvements in medicine from the uſe of new, or little known remedies. Will you allow me to mention, that I ſhould be glad to engage with an active and ardent young enquirer, who has already ſome readineſs in common chemical proceſſes. His employment would be in reſearches, connected with medicine and phyſiology. If ſuch a perſon, reſident in London, ſhould be induced by this notice to inquire, he might obtain ſome previous ſatisfactory information from Mr. Davy at the Royal Inſtitution.

Wiſhing your improved Journal the ſupport it merits,

I am, SIR,

Reſpectfully your's,

THOMAS BEDDOES.

IV.

On the Quantity of Nutriment to be obtained from various Kinds of Bones, and the beſt Method of extracting it, by Profeſſor PROUST.

Proust on improving the ſubſiſtence of the ſoldier.

MR. PROUST, profeſſor of chemiſtry at Madrid, has publiſhed a work, intitled an Inquiry into the Means of Improving the Subſiſtence of the Soldier. The following is a ſummary of what it contains of moſt importance, and of general utility.

Jelly from bones.

It has long been known, that a very agreeable and nutritious food might be procured from bones: a food particularly recommended as a reſtorative under the name of hartſhorn jelly, which is in fact procured by boiling ſhavings of bones, the common hartſhorn ſhavings of the ſhops. All that is wanting, is a cheap and eaſy mode of procuring this jelly in ſufficient quantity from a ſubſtance now commonly thrown away; for bones after they come out of the pot are as good for the purpoſe, and nearly as productive, as freſh bones. The ſhavings are too dear. Papin's digeſter, which has been employed, is a machine too expenſive for the poor, too dangerous for common uſe, and apt to give the jelly a burned taſte.

To

To obviate all these objections, nothing more is necessary, than to reduce the bones to powder; which may be very readily done between a pair of toothed iron cylinders, as in the ammoniac works. The bones thus comminuted are to be boiled in eight or ten times their weight of water for the space of four hours, or till about half the water is wasted, when the liquor will be found on cooling of a due gelatinous consistence. A vessel with a tight cover should be used, that the water may acquire as much heat as possible; and it should not be of copper, as this metal is easily dissolved by animal mucilage. Mode of procuring it.

It is to be observed, that bones from different parts afford different proportions of jelly, by which of course the quantity of water should be regulated. According to the experiments of Mr. P. five pounds of the middle part of the bone of a leg of beef will afford nine pints of jelly; the same quantity of the bone of the joint, fifteen pints; of the ribs and spine, eleven quarts; of the rump and edge bone, thirteen quarts. Five pounds of mutton bones of every sort together give nineteen pints of jelly. Pig's bones yield a little more. To Mr. P's taste, the jelly from pig's bones was the most agreeable of all: that from mutton had the distinguishing flavour of the meat. Of the jellies from beef bones that from the ribs was most pleasing, both to the sight and palate, that from the leg and joints least. Proportions of jelly in differen bones.

In warm weather the liquor must be boiled down somewhat more, if it be intended to assume the same gelatinous consistence when cold; as the same quantity of bone, that would afford a quart of jelly in winter, will not yield above a pint and a half, or a pint and a quarter in summer, but then it contains proportionally more nourishment.

If this jelly be boiled till it acquires a consistence a little thicker than a syrup, then poured out into plates, and when cold cut into pieces and dried on a net, it will keep a long time, and be particularly useful at sea. One ounce of this dry portable jelly, being soaked in water for a quarter of an hour to soften it, and then boiled, will make from a pint and a quarter to a quart of jelly, according to the season, and equally as good as that which is fresh extracted. Portable jelly

Mr. P. prepares a very pleasant restorative for the sick, as he informs us, by adding an ounce and a half of sugar, and a little salt, to fourteen or fifteen ounces of the jelly, and then

making it into an emulsion with twelve sweet and four bitter almonds, and a little orange peel.

Suet obtained from bones.

Another advantage to be derived from these refuse bones is the fat. Before grinding them to extract the jelly, Mr. P. chops them into pieces about an inch long with a cleaver, throws them into a kettle of boiling water, and lets them boil about a quarter of an hour. The fat obtained in this manner from sixteen pounds of rump and edge bones weighed, when cold, two pounds; and from the same quantity of the bones of the joints he obtained four pounds of solid fat. This, he observes, when fresh, may be used for various culinary purposes; and when it has been kept for some time exposed to the air, it becomes very good tallow for making candles.

V.

Observations on the Effects which take Place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr. ASTLEY COOPER.*

Injury or loss of the membrana tympani does not occasion deafness.

AFTER referring to a former paper, in which he had proved, by facts, that an aperture in the membrana tympani does not diminish the faculty of hearing, and that a complete destruction of the membrane does not occasion a total loss of that sense, Mr. Cooper observes, that there are various causes by which the membrane may be injured or destroyed. Of these the most common is suppuration in the meatus auditorius. In persons of a delicate constitution and irritable habit the ear-wax is liable to be hardened, gradually bringing on deafness, and then exciting inflammation and suppuration. If this be not remedied, not only the membrane lining the meatus, but that of the tympanum also, will be destroyed, the small bones of the tympanum be discharged, and sometimes considerable exfoliations take place. At Fig. 4, Pl. VI. is a view of the membrane of the tympanum lacerated by a blow on the head; probably from the air being driven into the meatus with violence. Children often introduce small stones, pieces of slate pencil, and even pins, into their ears,

Various causes of its loss or injury.

* Abridged from the Philosophical Transactions, 1801, p. 435:

in

in extracting which confiderable lacerations have been made in the membrana tympani. Fig. 5 fhews one torn in attempting to remove a pin.

How the membrane may be feen, and

The membrana tympani may be eafily feen in fome perfons, by directing the rays of the fun, or a condenfed light from a common lamp, into the ear: bnt this is not the cafe in all; for the meatus differs confiderably in different perfons, both in its depth and diameter. If the ear be clear of wax, the membrane has a bright tendinous appearance; and an aperture in it appears as a dark fpot, which, by the filvery furface of the membrane furrounding it, is rendered diftinctly perceptible. If there be an aperture, air alfo, upon blowing the nofe with violence, will be forced with a whiftling noife through the ear; the fmoke of tobacco may be driven from the mouth through the ear; or water may be injected from the ear into the throat.

its appearance.

Means of difcovering an aperture in it.

Effects of the deftruction of the membrane.

If the whole of the membrane be deftroyed, and three out of four of the fmall bones of the tympanum be removed, an almoft total deafnefs enfues; but the ear, after a time, begins to recover its powers, and, in the end, regains them, with that degree of imperfection only, which was defcribed in the cafe of Mr. P——, in Mr. Cooper's former paper. When the membrane of one ear only is deftroyed, a greater degree of deafnefs takes place in that ear, than would happen in either, were the membrane deftroyed in both. This probably arifes from the difufe, into which the imperfect ear falls. Thus Mr. G——, at an early period of life, loft a great portion of the membrana tympani of the left ear; and as he heard fomewhat better with his right ear, he was little in the habit of employing the left, till at length he confidered himfelf almoft totally deaf in it. Becoming deaf in the right ear, however, and confequently obliged to employ the left, he found it by no means deprived of its powers.

Fig. 3.

Cure of a fpecie of deafnefs.

Thefe obfervations have induced Mr. Cooper to try the effect of an operation, which has proved fuccefsful in feveral inftances of one fpecies of deafnefs,—that which arifes from obftruction of the Euftachian tube. Of this obftruction there are feveral caufes. A common cold, affecting the parts contiguous to the orifice of the tube, may prevent the free paffage of the air into the tympanum. The deafnefs thus occafioned is in general temporary; but it may become permanent,

The Euftachian tube may be obftructed from feveral caufes.

manent, if a frequent recurrence of ſuch attacks produce permanent enlargement of the tonſils. The ſcarlet fever occaſions ulcers in the throat, which, in healing, frequently cloſe the Euſtachian tubes. A venereal ulcer may have the ſame effect. The tube has been cloſed by an extravaſation of blood in the tympanum. And in one inſtance a ſtricture in the tube rendered the paſſage extremely difficult. In this caſe, by forcing air from the mouth into the cavity of the tympanum, and then preſſing gently on the ear to expel part of the air from the cavity, an immediate increaſe of the power of hearing was produced. Theſe are the moſt common cauſes of obſtruction in the Euſtachian tube; and Mr. Cooper has reaſon to think, from the experience he has already had, that they may all be remedied by puncturing the membrana tympani. In ſupport of this opinion he gives four ſelect caſes, one of which is the following:

Caſe of deafneſs cured by puncture of the membrane of the tympanum.

Mr. Round, of Colcheſter, conſulted Dr. Baillie reſpecting his ſon, Mr. John Round, aged ſeventeen, who had laboured, from his birth, under ſuch a degree of deafneſs, as would have incapacitated him from engaging in buſineſs. Dr. Baillie, having ſatisfied himſelf that there was no nervous defect in the ear, referred him to Mr. Cooper. He found that this gentleman had been born with an imperfect ſtate of the fauces, which rendered him incapable of blowing his noſe; that the Euſtachian tubes had no openings into the throat, and, therefore, that he was unable to force air from the mouth into the ear. The auditory nerves, however, were perfect; for he could diſtinctly hear the beating of a watch, if placed between the teeth, or againſt the ſide of the head; and he had never perceived any buzzing noiſe in his ears. Our author therefore adviſed him to ſubmit to the operation of perforating the membrana tympani; to which he cheerfully conſented. The moment this was done, a new world was opened to him; and the confuſion produced by the number of ſounds, which immediately ſtruck his ear, made him ſink upon a chair, almoſt in a fainting ſtate. From this ſtate he recovered in about two minutes; and, finding that his hearing was completely reſtored upon the one ſide, he wiſhed the operation to be performed upon the other; which was immediately done, with the ſame happy reſult, and without his experiencing the ſame confuſed ſenſation as before. Near two months after the

operation,

operation, Mr. Cooper had the pleaſure to receive an aſſurance from him, that he had ſuffered no relapſe, nor any inconvenience from the openings which had been made, and that his hearing continued perfect.

Mode of performing the operation.

The operation conſiſts in paſſing into the ear a canula, of the ſize of a common probe, in which a trocar is concealed: the canula is to reſt upon the membrana tympani, and the trocar is then to be thruſt through the membrane. The trocar ſhould be ſo adjuſted, as not to paſs more than one-eighth of an inch beyond the canula, to prevent its reaching the oppoſite ſide of the cavity of the tympanum. Should it however touch the perioſteum of the tympanum, it can be productive of no ſerious harm. The aperture ſhould be made in the anterior and inferior part of the membrane, under the handle of the malleus, which muſt not be injured in the operation; and it is therefore neceſſary, that the operator be acquainted with the exact ſituation of this bone. Though the membrana tympani is vaſcular, the veſſels are ſo ſmall, that they bleed but little; and therefore, if much blood be diſcharged, the operation cannot have been properly performed. In an ear otherwiſe healthy, the operation is attended with ſo ſlight a degree of pain, that, when it has been performed in one ear, the patient expreſſes no unwillingneſs to ſubmit in the other. The ſenſation it occaſions is momentary; and no ſubſequent inconvenience of any kind ariſes: though if the ear have been previouſly irritated by ſtimulating applications, the operation will be painful, and ſhould be deferred, till the inflammation has ſubſided.

Caſes in which it affords relief.

As this operation affords relief only in caſes of cloſure of the Euſtachian tube, attention ſhould be paid to the following criteria of this defect.

Criteria for diſtinguiſhing them.

If a perſon, on blowing the noſe violently, feel a ſwelling in the ear, from the membrana tympani being forced outward, the Euſtachian tube is open. Though the tube be cloſed, if the beating of a watch placed between the teeth, or preſſed againſt the ſide of the head, cannot be heard, the operation cannot relieve, as the power of the auditory nerve muſt have been deſtroyed. In a cloſed Euſtachian tube there is no noiſe in the head, like that accompanying nervous deafneſs. Laſtly, it is right to inquire, whether the deafneſs were immediately preceded by any complaint in the throat.

The

The caufes of deafnefs are numerous, and many cafes may be relieved by furgery.

The caufes of deafnefs are extremely numerous; and many of thofe which affect only the meatus auditorius, the membrana tympani, the cavity of the tympanum, and the Euftachian tube, admit of relief from furgery.

Nervous deafnefs.

But there is one fpecies, in which it would be abfurd to expect this from any operation upon the membrana tympani. This occurs more frequently than any other, happening generally in old perfons; but fometimes, in the delicate and irritable, in the earlier ftages of life. Anxiety and diftrefs of mind have been known to produce it. Its approach is generally gradual: the perfon hears better at one time than at another; a cloudy day, a warm room, agitated fpirits, or the operation of fear, will produce a confiderable diminution in the powers of the organ. In the open air the hearing is better than in a confined fituation; in a noify, than in a quiet fociety; in a coach when it is in motion, than when it is ftill. A pulfation is often felt in the ear; a noife, refembling fometimes the roaring of the fea, and at others the ringing of diftant bells, is heard. This deafnefs generally begins with a diminifhed fecretion of the wax of the ear, which the patient attributes to fome unufual expofure of the head to cold; and this continues as long as the difeafe remains. It may be cured in the commencement by the application of fuch ftimulants, as are capable of exciting a difcharge from the ceruminous glands; for which purpofe they fhould be introduced into the meatus. But if thefe be ufed fo as to irritate, without exciting a difcharge, they are rather prejudicial than otherwife. In cafes of this kind the operation has afforded no farther relief than diminifhing the noife in the head.

May be cured in the commencement by ftimulants.

Deafnefs from the generation of a folid fubftance in the labyrinth.

Another caufe of deafnefs is probably irremediable. This is the generation of a folid fubftance in the labyrinth of the ear, inftead of the water with which it is naturally filled. Mr. Cline, diffecting the head of a young man born deaf, and confequently dumb, found all the parts of the organs of hearing perfectly formed, but the veftibule, cochlea, and femicircular canals, were filled with a fubftance of the confiftence of cheefe.

Thefe inftances of deafnefs Mr. Cooper thought it right to defcribe, becaufe they are liable to be confounded with that which arifes from a clofed Euftachian tube.

EXPLANATION OF THE FIGURES. Plate VI.

Fig. 1 ſhews the external ear, the meatus auditorius, membrana tympani, and Euſtachian tube.

A, the meatus.

B, the membrana tympani.

C, the cavity of the tympanum.

D, the Euſtachian tube.

Fig. 2 ſhews the perforating inſtrument as it is introduced in the operation.

Fig. 3 the membrana tympani of Mr. G——, of which only that part which appears of a lighter colour remains.

Fig. 4 the membrane lacerated by a blow.

Fig. 5 the membrane lacerated in an attempt to extract a pin.

Fig. 6 ſhews the membrana tympani of a medical man in the city, having a fungus projecting through it. In this ear he is conſiderably deaf.

Fig. 7 the other membrane of the ſame gentleman.

Fig. 8 one of the membranes of Mr. P——, whoſe caſe was deſcribed in the former paper.

Fig. 9. A membrana tympani in its natural ſtate, ſhewing the attachment of the manubrium to the malleus.

Fig. 10 the appearance of the membrane after having been punctured.

VI.

Note reſpecting the Abſorption of Nitrous Gas, by Solutions of Green Sulphate and Muriate of Iron. By Mr. H. DAVY, *Director of the Laboratory, and Lecturer on Chemiſtry to the Royal Inſtitution.—(From the Author.)*

WHEN nitrous gas is brought in contact with ſolution of green ſulphate, or green muriate, of iron, it is rapidly abſorbed; the colour of the fluid alters; and it becomes, when ſaturated with the gas, dark brown, and almoſt opaque. Solution of green ſulphate, or muriate of iron, abſorbs nitrous gas;

Solution of ſulphate, or muriate, of iron, impregnated with nitrous gas, apparently undergoes no change at low temperatures, when preſerved from the contact of the atmoſphere. and undergoes no change at low temperatures.

But

unless expofed to the air, when it abforbs oxigen, becomes acid, depofits red oxide, and affords a little ammonia.

But if it be expofed to it, it rapidly abforbs oxigen, lofes its colour, and becomes acid to the tafte. In this cafe, the green oxide of iron is converted into red oxide; and a fmall quantity of ammonia is formed.

The impregnated folution, in vacuo, emits its nitrous gas without change.

When a folution of fulphate of iron, impregnated with nitrous gas, is placed under the receiver of an air pump, in proportion as the preffure of the atmofphere is removed from it, it gives out its nitrous gas. When the mercury in the gage ftands at about $\frac{1}{10}$ of an inch, it becomes almoft wholly freed from it, recovers its colour, and is found unaltered in its properties.

The fame folution, heated, emits nitrous gas, depofits a little red oxide, and contains ammonia.

When folutions of green fulphate, or muriate, of iron, impregnated with nitrous gas, are expofed to the heat of a fpirit lamp, nitrous gas is produced from them in a very pure ftate, the fluids lofe their dark colour, and a fmall quantity of yellow oxide of iron is thrown down in them. If examined after this procefs, they are found to contain a little ammoniac, combined with fome of the acid; and the oxide of iron diffolved is green oxide.

Theory.

The theory of thefe appearances is obvious.

The nitrous gas is firft abforbed by fimple affinity; probably from the attraction of the iron for oxigen.

The abforption of nitrous gas, by folutions containing oxide of iron, at its minimum of oxidation, at common temperatures, appears to be owing to a fimple combination between the gas and the fluid; owing probably in fome meafure to the modified affinity of the green oxide of iron for oxigen; for the red fulphate and muriate of iron have no affinity for nitrous gas.

Expofure to air converts the nitrous gas into acid; a portion of which, and alfo of the water, are decompofed by the iron converted into red oxide.

The changes taking place in the impregnated folutions, when expofed to the atmofphere, evidently depend upon the converfion of the nitrous gas of the folution into nitrous acid, by the abforption of oxigen; and upon the fubfequent decompofition of a portion of this nitrous acid, and of the water of the folution, by the green oxide of iron.

Heat increafes the tendency of nitrous gas to fly off, and alfo that of the iron to decompofe it, and water. Hence red oxide is formed by the oxigen of this gas and of the

Heat feems to act upon impregnated folutions, by increafing the tendency of nitrous gas to affume the elaftic form, and by increafing the affinity of the green oxide of iron for oxigen. In confequence, a portion of nitrous gas flies off, and another portion is decompofed; its oxigen combining with the green oxide, and its nitrogen with the hidrogen of a fmall quantity of water, which is acted upon by a fimilar attraction.

The abforption of nitrous gas by folution of common fulphate of iron was difcovered by Dr. Prieftley. It has been employed

employed by Mr. Humboldt for the purpose of ascertaining the quantity of nitrogen mingled with nitrous gas; and that experimentalist, in conjunction with Mr. Vauquelin, has attempted to prove that the process of absorption is owing to an immediate decomposition of the nitrous gas by the water of the solution; in consequence of which nitrate of ammonia is formed.

water, and ammonia by its nitrogen with the hidrogen.

History. Humbold and Vauquelin think the gas is completely decomposed at its first absorption, and ammonia formed.

This conclusion is published in the 28th volume of the *Annales de Chimie*. In the 39th volume of the same work, 30 Messidor, An 9. p. 1. Mr. Berthollet states, that nitrous acid is probably formed during the absorption of nitrous gas by solution of sulphate of iron; in consequence of the evolution of a portion of its nitrogen, and in consequence of the more intimate union of its remaining constituent principles. Yet, in the same paper, he details an experiment on the application of heat to an impregnated solution, which was attended with results similar to those described above; and in consequence of which he is induced to enquire, "If the sulphate of iron is capable of exerting two different actions in nitrous gas; one tending simply to absorb it, and the other to decompose it?" p. 11.

Berthollet apprehends a partial decomposition;

but nevertheless is led to suspect the differences of action from temperature.

The general account that has been given in this Note of the true nature of the process, is extracted from "Researches, Chemical and Philosophical, concerning Nitrous Oxide, published in June, 1800." The experiments upon the absorption were made under Mercury; and, in consequence, the agency of the atmosphere, which appears to have misled the celebrated foreign chemists just quoted in their conclusions, was avoided.

The author's experiments were made over mercury; and consequently he was not, like those chemists, misled by the action of the atmosphere.

VII.

On certain Metallic Sulphurets. By PROUST, *Professor of Chemistry at Madrid* *.

I LONG supposed, that the iron in pyrites was oxided at the minimum, and, supporting my opinion by some analogies, I published it in the year 1795; but when my attention was recalled to the subject, by reading the Memoir which Vau-

Ill founded opinion, that the iron in pyrites is oxided.

* Journal de Physique. Thermidor. 9.

quelin

quelin has published on the sulphurets, I thought I ought no longer to delay making known the facts which have induced me to adopt a different opinion. I made use of the fine cubical and dodecahedral pyrites, which are obtained in abundance from an indurated argill, found in the vicinity of San Pedro Marino, near the town of Soria.

Distillation of pyrites

Four hundred grains of entire pyrites of the size of hemp-seed, distilled by a red heat, yielded,

Of Pure sulphur	-	75 grains
Residuum	-	322
Loss	-	3
Total		400

Four hundred grains of the same of a little larger size, yielded,

Sulphur	-	81 grains
Residuum	-	314
Loss	-	5
		400

A moderately red heat is sufficient for extracting all the sulphur which they are able to yield. In the first moments, a small quantity of aqueous vapour passes over; afterwards, two gases, which may easily be recognised by their smell, namely, the sulphureous and hydrogen-sulphureous, which mutually decompose each other in the water of the receiver, and render it milky. Both are owing to the decomposition of the humidity; the sulphur passes over afterwards, and the gas is no longer perceived.

Converts them into sulphuret.

The abstraction of about 20 parts in 100 of the sulphur from the pyrites, destroys in a great measure their metallic lustre and solidity; and though they preserve their form, they are more voluminous, melted on all sides, and may be crushed between the fingers; they have only the dull colour of an artificial sulphuret.

They contain no oxigen.

Hence it is evident, on comparing the products, that if oxigen constituted part of the pyrites, we must seek it in the residuum of their distillation; but the following facts annihilate the hope of finding it there.

Proof.

When we treat the different oxides of iron with sulphur or cinnabar, in a red heat, the results are, sulphureous gas, and common sulphuret of iron. If therefore the pyrites contained oxigen,

oxigen, it is evident that the latter could not escape the attraction of the combustible substance in a high temperature.

When treated with carbon, pyrites produces only sulphurated hidrogen; but if first deprived of 12 or 20 hundredths of of sulphur, it presents only sulphuret, or iron saturated with sulphur, in the proportion of 60 to 100. It is soluble in the sulphuric, muriatic, and dilute nitric acids, affording sulphurated hidrogen in abundance.

Pyrites is a sulphuret overcharged with sulphur.

Pyrites, therefore, according to these results, is nothing more than a sulphuret formed by nature in the humid way, and surcharged with an excess of sulphur, as if to ensure the duration of her work.

They differ much; and only yield their surplus by distillation.

The pyrites will undoubtedly be found to differ greatly from each other with respect to this excess of sulphur; for, according to Henkel, there are some which yield 25,28, even 32 per cent. of sulphur. It is therefore to this surplus that the product of this distillation belongs, and not to the sulphur, which has been combined with iron by the invariable law of the proportions; for the metal, when saturated with $\frac{60}{100}$ of this combustible, yields none on being exposed to that temperature which deprives the pyrites of its excess.

Easy method of procuring hidrogen.

The pyrites, besides their different known uses, are of great use in a laboratory. We may fill a crucible with them, cover the powder with about a third part of filings, sprinkle over these a little charcoal powder, and afterwards bring the whole to a red heat, without carrying it so far as to reduce them to the state of fusion. In this manner we obtain a homogeneous mass, very convenient for procuring hidrogen in abundance.

The least sulphurated pyrites are soonest vitriolized.

By what I have just said, I do not however mean to imply that all the pyrites resemble each other. Those, for example, which possess the property of becoming vitriolized, approach perhaps the nearest to the sulphuret of iron without excess; for it is certain that the pyrites most charged with sulphur are at the same time those which resist the tendency of the elements to destroy them the longest.

The pyrites of Soria contains a little chalk, sand, and argill, but not the slightest trace of copper: its solution in the nitric acid is reduced to the minimum of oxidation by means of sulphurated hidrogen; and the yellow powder which separates from it is nothing but sulphur.

The

Natural production of red ochre. The pyrites undergo a slow decomposition, by which oxigen is substituted in the place of the sulphur, without their losing any thing of their dimensions; they are then nothing more than red oxide. When the waters transport, divide, and attenuate the oxided pyrites, they become changed into red ochre: of this kind is that which in Spain is termed *Almagné*, from the name of the village where it is found. It is employed for painting houses, marking sheep, colouring the Seville snuff, polishing glass, &c. This earth, when distilled, yields sulphureous acid in abundance: it announces its origin by the remnant of sulphur which it contains.

Yellow ochre. As to the beautiful yellow ochres, which acquire a red colour by merely losing their humidity, they proceed from the spontaneous decomposition of the spathose ores: we therefore find them to contain lime and manganese. I have observed this conversion of the spathose ore into the state of yellow ochre in a very distinct manner in certain veins; that is to say, the oxides of iron and manganese, when both at the minimum, and saturated with carbonic acid, raise themselves to the maximum, by abandoning the acid, which can no longer remain united with them in this state.

Umber.

Analysis of umber shews it to be vegetable. If this earth be a vegetable residue, as cannot be doubted, its origin is well confirmed by its analysis, for it is a compound of oxide of iron and manganese, both at the maximum, of argill, of sand, &c. As to the argill, it abounds in the ashes of certain kinds of wood; I have found it in large quantity in that of the olive and careb trees; in that of the holm (*encina* in Spanish) I have found the calcareous phosphate, which I neglected to announce at the proper time. In addition to my second memoir on Prussian blue, I have to add, that ox's blood certainly contains manganese.

Sulphuret of Mercury.

Mercury in cinnabar is not oxided. Artificial cinnabar constantly yields 85 per cent. of mercury on the one hand; on the other it relinquishes 14 and $14\frac{1}{2}$ of sulphur to the antimony which is used for effecting its decomposition. The mercury is not therefore oxiginated in cinnabar.

In

In my youth I made two experiments, which might have coft me very dear. I wifhed to convert mercury into cinnabar in the dry way, as I had done by employing the oxides in the humid way. Four ounces of corrofive muriate, and four ounces of red oxide, triturated with fulphur, were expofed to the fire, each mixture in a feparate retort. In lefs than half an hour, two explofions took place, which were heard throughout the extenfive works of La Salpêtrière. My brother, who conducted the firft experiment, being fuddenly overwhelmed with a thick fmoke of fublimate, had nearly loft his life, and his lungs were long afterwards difordered. The refult of my operation was, that the dome of the furnace was thrown into the air, whilft the door of the afh-pit was fhattered againft a wall, and narrowly efcaped ftriking me in the ftomach.

Dangerous explofions, by attempts to form cinnabar with oxided mercury, by heat.

The powder which arofe from the detonation of the oxide mixed with fulphur, according to the method of Bayen, is cinnabar of a violet-red colour.

The æthiops, prepared with the aid of fire, reddens fpontaneoufly in courfe of time, in the oxiding veffels.

Mercury poured into a retort with melted fulphur always occafions an inflammation; but if both have been previoufly heated, the union takes place, and the æthiops affords cinnabar without flame. As to that which accompanies the procefs ufed by the Dutch, it deferves to be better examined than has hitherto been done.

On the inflammation of the common procefs.

Hydro-fulphurated water, poured gradually into a folution of fublimate, decompofes it entirely. The precipitate is mild muriate, and muriatic acid is difengaged. This refult proves, that there is not only more oxigen in the oxide of fublimate, but alfo, as Berthollet has remarked, more marine acid. If, on the contrary, we pour the folution of fublimate into the hydro-fulphurated water, the whole becomes precipitated in the form of æthiops, and the muriatic acid remains alone.

Corrofive fublimate contains more oxigen and more acid than the mild muriate.

Remarkable fact.

The name of *fweet* muriate brings to my recollection a fact recorded by Lemery in the Memoirs of the Academy. He knew an alchemift, who eat the mild muriate like bread; he has feen him fwallow four ounces at a time, and he affured him that he took from time to time a like dofe, to purify his blood, as he faid. This anecdote, the authenticity of which cannot be fufpected, induces me to afk what we are to think of the 18 and 24 grains of *mercurius dulcis* which phyficians are

Anecdote, to fhew the inefficacy of mild muriate as a medicine.

It was not the muriate. W. N.

are in the daily habit of prefcribing? I greatly fear that it may be much the fame with regard to its effects as with thofe of fedative falt, and fo many other medicines, which have nothing to recommend them, but the honour which is done to them, of affording them a place, from age to age, in our difpenfatories and catalogues of the *materia medica*.

Mild muriate treated with fulphur;

I find alfo in my notes of the fame period, that the mild muriate, treated with fulphur in a retort, affords cinnabar and corrofive muriate; the mild muriate is therefore divided into two portions; one of which is converted into cinnabar, and the other charges itfelf with the oxigen and acid of the firft.

alfo turpeth.

That turpeth mineral, treated in the fame manner, is changed into cinnabar, and quits the oxigen, which goes to form fulphureous acid.

The concentrated fulphuric acid decompofes cinnabar, like the pyrites and other fulphurets, yielding to the metal the oxigen which they require; hence the fulphureous acid, &c.

Cinnabar in the humid way.

Mercury prefers fulphur to oxigen.

Mercury having much more affinity for fulphur than for oxigen, never fails to fhew its preference for this combuftible. It is not fubject to the law which enables zinc, tin, antimony, &c. to combine with fulphur, without abandoning the oxigen.

Liquid fulphurets decompofed by crude mercury;

F. Hoffman was, I think, the firft who thought of decompofing the ammoniacal fulphuret by means of mercury. Baumé and Wiegleb have fince occupied themfelves with the fubject, and in a more extenfive manner.

which takes the fulphur, and becomes cinnabar.

Mercury, poured into flafks containing fulphuret of potafh, or of ammonia, decompofes them entirely, and reduces them to potafh and ammonia: in the fpace of a few days it paffes from the black to the red colour. If mercury required oxigen in order to affume the colour of cinnabar, whence fhould it take it? This procefs is certainly of a very difoxidating nature.

Mercurial falts and oxides alfo.

The nitrates, muriates, fulphates, and mercurial oxides of every kind, all yield æthiops in the firft moment of their mixture with the fulphurets: they all become violently heated. In order to accelerate their coloration, it will be fufficient to place the flafk with the mixture upon burning coals; in an inftant they begin to grow red at the points where the heat exerts its action. Is it fulphurated hidrogen which they then lofe? For my part, I cannot tell.

All

All these cinnabars differ with respect to their shades of colour and fineness; some are of a tinge approaching to purple, violet, &c. That of sublimate combines with a great degree of tenuity, a vivid scarlet brilliancy. It is much superior to the richest vermilions, and it is to be wished that painters might become acquainted with it.

These cinnabars differ. A very beautiful pigment.

Others are pulverulent, of a dark or dull colour; such are those which the sulphuret of potash yields; they have neither the lustre nor the tenuity of the others, for they are crystalline. For the rest, all these cinnabars present no extraordinary appearances in sublimation; by this process they lose the brilliancy which they owed to their state of division.

Tin, which has so great an affinity with oxigen, takes however nothing else than sulphur from cinnabar; if the latter contained also oxigen, it would take it from it with the same facility as it does the sulphur; and the presence of the one would certainly be no impediment to the tin becoming charged with the other: for if we heat a mixture of cinnabar and oxide of tin at the maximum, we obtain mercury, sulphurated oxide of tin, and sulphureous acid. This last is here produced, as in the case where sulphur is treated with the oxide of tin, by a reduction which brings this oxide from the maximum to a minimum of oxidation, of which I do not yet know the numerical value.

Tin takes sulphur from cinnabar; but no oxigen: that principle not being present.

Sulphuret of Arsenic.

This is also one of those which have long deceived me by the analogy which its transparency and colour seems to indicate.

The sulphurets of arsenic contain no oxigen.

The arsenical acid and white oxide, treated with sulphur, lose their oxigen, yield sulphureous gas, and are reduced into a sulphuret, which is transparent, vitreous, of a yellowish red colour, and capable of being raised in distillation.

Pure arsenic, treated in the same manner, yields the same kind of sulphuret, of perfect volatility, colour, and transparency.

In whatever ways we may expose these sulphurets to the action of pulverised charcoal, iron, tin, &c. they remain unalterable. Minute portions of sulphureous gas, and sulphurated and arsenicated hidrogen, appear at first in consequence of the moisture, but nothing more.

The native, ſcaly, flexible orpiment melts quietly, and forms a vitreous, red, tranſparent maſs, reſembling realgar, and unalterable by the action of charcoal. There may exiſt natural ſulphurated oxides of arſenic, but certainly theſe are not ſuch as are formed by an elevated temperature. The native realgar of Ronda in Andaluſia, is alſo a metallic ſulphuret without oxigen.

Sulphuret of Copper.

The ſulphuret of copper exhibits a ſecond inſtance in the mineral kingdom of ſulphurets with a ſurcharge of ſulphur.

Characters of ſulphuret of copper.

As the lights of analyſis have hitherto been wanting, in order to point out to mineralogiſts the real characters of this ſulphuret, they have always deſcribed it in an imperfect manner, or confounded it with other mineral productions. In its pureſt ſtate, its colour is always a deep blue, violet, or mixed with the copper tinge of indigo, if rubbed with a ſmooth ſubſtance. Its colour is liable to be diſguiſed by a mixture of carbonate of copper, red oxide of iron, &c. But when we ſeparate theſe by ſolution, the ſulphuret is reſtored to its real colour.

Variations from impurity.

More commonly the ſulphuret of copper is diſguiſed by its intimate combination with the other metallic ſulphurets.

Combined, for example, with the ſulphuret of iron, it affords the copper-coloured pyrites, in which the exiſtence of the blue ſulphuret may eaſily be demonſtrated.

If with theſe two ſulphurets, conſidered as the baſe, are combined thoſe of antimony, lead, arſenic, mercury, ſilver, zinc, &c. each ſeparately, there reſults a ſeries of ores of copper, rendered grey by as many different metals, but which do not on that account, as mineralogiſts believe, neceſſarily contain ſilver.

Compound ores of copper.

If with the two firſt ſulphurets we combine in idea the 2d, 3d, 4th, and 5th of theſe which we have juſt mentioned, we have in theſe combinations as many complicated ores of copper. Amongſt thoſe which are brought from America, there are others likewiſe compounded with native ſilver, the carbonates of iron, manganeſe, ſulphate of barytes, &c. Such are the different ſpecies of mineralizations which mineralogiſts have indiſcriminately comprehended under the two claſſes of white and grey ores of copper.

The

The grey ore of copper of La Cren, in the kingdom of Valentia, is of this kind: it is composed of four sulphurets, that of copper, of iron, of antimony, and of mercury. Deposited in masses in the calcareous breccias formed by the remains of mountains which no longer exist, it undergoes a decomposition at its surface, which converts three of the sulphurets into oxides, whilst that of mercury remains entire, and imparts to the decomposed ore a brilliant vermilion hue. At the centre there is always a nucleus composed of these four sulphurets in their entire state. The analysis of this mineral, given by Fernandez, is in the *Annales*: but I return to the sulphuret of copper, from which I did not intend to digress.

Those which I have had opportunity to examine contain from 14 to 15 per cent. of sulphur, which is easily extracted from them by a moderate heat, and what remains in the retort is always the blue sulphuret, saturated according to an uniform proportion. This proportion is 18 per cent. like that of the artificial sulphuret. If a native sulphuret of copper leaves 86 of residuum, we may precipitate its nitric solution by sulphurated hidrogen; and the precipitate separated from the excess of sulphur re-produces 86 parts of sulphuret of copper. Hence we see, that though the native sulphuret is subject to an excess of sulphur, it does not on that account differ from the artificial sulphuret, when it has been deprived of this excess.

Copper pyrites, like that of iron, contains an excess beyond the state of sulphuret.

By the action of a red heat, the oxides of copper, mixed with sulphur, yield only blue sulphuret. This metal does not afford a sulphurated oxide, either in nature or in the products of art. Its formation is always attended with a considerable extrication of heat. It is astonishing, if it be duly considered, that it could ever for one moment have been mistaken for a combustion.

Copper sulphurets contain no oxigen.

The blue sulphuret dissolves in copper, and forms the black coppers, independently of the iron which they may contain. Acids of 10 or 12 degrees separate it from these coppers without decomposing it.

Black copper.

On the Fuming Muriate of Tin.

Since the year 1777, I have been in the habit of preparing this muriate with corrosive sublimate and pulverised tin; and though I never thought of publishing this process, it was because I imagined it to be in common use in every laboratory. But

Fuming muriate of tin prepared without amalgamation.

But as, in the lateſt treatiſes of chemiſtry, the ancient proceſs of amalgamation is ſtill preſcribed, I think it may be uſeful at preſent to recommend the uſe of the mere pulveriſed tin.

Theſe are the details which I find in my notes of that period; they ſhew that the following proportion is the beſt:

Inſtructions. Twenty-four ounces of ſublimate and eight ounces of tin yield nine ounces of fuming liquid.

Conceiving that ſome advantage might be obtained by augmenting the proportion of ſublimate, on account of the great exceſs of pure and ſemi-oxided tin which is found in the reſiduum, I made the following trial:

Experiment. Thirty-two ounces of ſublimate and eight ounces of tin yielded ten ounces of the liquid; hence it appears, that the expenditure of eight ounces of ſublimate is not compenſated by more than one ounce more of liquid.

The acid is not oxided in the fuming muriate. I ſhall ſhew, upon another occaſion, that in the fuming muriate of tin, as well as in the corroſive muriate of mercury, the baſes only are oxidated at their maximum, whilſt the muriatic acid remains in its ordinary ſimple ſtate.

VIII.

A Memoir on the Method of Bleaching the Paſte of Paper. *By* CITIZEN LOYSEL *.

Bertholllet's bleaching proceſs. THE advantages of the method of Citizen Berthollet for bleaching thread and piece goods, by means of the oxigenated muriatic acid are univerſally known. Citizen Chaptal has uſefully applied this invention, to reſtore the colour of prints and printed books; and he has alſo ſimplified one of its moſt important parts in his new proceſſes reſpecting the art of applying the lixivium.

Advantages of applying it to paper. The adoption of Citizen Berthollet's method to the art of paper-making, is capable of carrying this branch of induſtry to a ſtill greater degree of perfection, particularly in France. We are abundantly ſupplied with the raw materials proper for manufacturing paper; but according to the preſent proceſſes of our paper mills, there is only a very ſmall portion of the

* Tranſlated from the Annales de Chimie XXXIX. 137.

rags which can be ufed to manufacture fine white paper. All the refidue is condemned to the fabrication of papers of inferior quality.

The procefs of bleaching the pafte of the paper maker, even when produced from the moft common rags, will communicate to it the quality of the beft fort. By thefe means our paper manufactories may fupply our wants in fine white paper, and even obtain the preference in foreign markets. The refult of this operation would be, that a greater number of workmen would find employment, and the advantages of this increafe of induftry would be of ftill greater national value, than even the foreign export which might be expected.

The fuccefs of bleaching the pafte of paper by the method of Citizen Berthollet is no longer problematical. The application which has been made to the paper ufed in making affignats, has placed this queftion beyond all doubt as to its folution. Actual trial and practice.

It was at the commencement of the year 2, that the committee of affignats and monies of the National Convention, of which I was a member, refolved to employ this method, together with that of ftereotypage which had been adopted, to oppofe new obftacles to the practice of forgery. In making paper for affignats

We particularly confulted Citizens Berthollet, Fourcroy, and Guyton on this enterprize. Their approbation of the project, and the information they afforded us, foon gave us the power of realifing it. We were alfo affifted with the knowledge of Citizens Welter, Athenas, Alban, Carny, Marchais and Ribaucour, who with great zeal communicated their proceffes, and permitted us to infpect their feveral manufactories.

I fhall not here relate all the previous experiments we made before we eftablifhed our works on a large fcale, but fhall content myfelf to point out concifely the manner of our commencement, the agents we employed, and the means which to us appeared the moft fimple to obtain our defired purpofe. I fhall afterwards add a few obfervations refpecting the means of ftill greater perfection, which the advancement of the fcience fince that time has rendered the fame operations capable of. Order of narration.

Our firft proceffes were executed precifely according to the method of Citizen Berthollet. The rag was fubjected in The rags were bleached at the firft trial.

fucceffion

fucceffion to different lees, to baths of the bleaching liquor, and fulphuric acid pointed out in his memoir. Berthollet had fhewn, and we were alfo convinced by our own experience, that the gas is lefs confined in the fimple fluid, prepared without addition of fixed alkali, than it is in that which contains potafh or foda; and that it is confequently more difpofed to feparate and enter into new combinations. We therefore at firft made ufe of this fimple liquor; but the workmen foon exhibited a ftrong repugnance to its ufe on account of the fumes it emits, which are extremely inconvenient, even when chalk is diffufed in the liquor. This inconvenience forced us to abandon it, though with regret. This facrifice was fo much the more confiderable, as it occafioned a lofs of time, and confiderable increafe of expence. We decided that we would receive the gas in a folution of potafh; but as the dofes in which this alkali may be ufed have limits of great extent, we endeavoured to keep as near as poffible to that preparation which is fufficient to prevent the fpontaneous difengagement of the gas, and by that means caufe the liquor to lofe the odour we were defirous of avoiding. This dofe was 5 kilograms of potafh to 100 litres of water, (11 pounds avoirdupoife, to $21\frac{1}{2}$ ale gallons.)

Difficulty of ufing the bleaching liquor without alkali.

The rags bleached in this manner became of the moft brilliant white. Neverthelefs, a part of this perfection difappeared, when the rag was converted into pafte, and that pafte into paper. Is was eafy to difcover the caufe; namely, that the interior parts of the thread in the rag were lefs expofed to the action of the liquor than thofe at the furface. This motive determined us to abandon the bleaching of the rags, and to operate upon the pafte itfelf.

The rags became very white, but when opened by the mill were lefs fo.

We were here oppofed by new obftacles. When the rag is converted into a pafte proper to be worked, its coherence is fuch that it fettles, and no longer permits the lees and baths of the bleaching liquor to penetrate through all its parts, in confequence of which property the paper was found to have veins and different fhades of colour. We remedied this inconvenience, by taking the matter in a mean ftate between the rag and the pafte proper to be converted into fheets of paper. We fucceeded in this refpect by deftroying the texture of the rag under the firft cylinder fo as to feparate its fibres, an operation which ufually lafted two hours for a pile of 50 kilograms.

The pafte was therefore bleached

In its half prepared ftate.

Thus

Thus it was, that by fucceffively avoiding the extremes of too much and too little mechanical connection, we advanced towards our object.

The apparatus which Citizen Welter imagined, and of which Citizen Berthollet has given a defcription in the firft volume of the Journal of Arts and Manufactures, is applicable to all the methods which can be employed to procure the different kinds of bleaching liquor, whether the water of the receiver contain fixed alkali or not; whether the muriatic acid be ufed on the oxide of manganefe, or the gas be obtained by fulphuric acid, upon the mixture of oxide of manganefe and muriate of foda. This apparatus is particularly preferable to all others in the cafe where the water of the receiver contains no alkali, becaufe the abforption of the gas is favoured by its being brought into contact with the water at a great number of furfaces. But as we had determined to ufe a folution of potafh, we were able to make fome modifications of this apparatus. Apparatus of Welter for ox. mur. acid

1. The three inner partitions of the receiving veffel were reduced to one only. Ours was formed of a tub containing another tub inverted, and both were covered with fheet lead. was confiderably fimplified.

2. The fize of the tubes of communication was fo confiderable, that we had nothing to fear from abforption during the courfe of the diftillation.

3. Laftly, the practical fkill of our workmen in managing the fire, and the advantage of having only one opening to lute enabled us to fuppress the intermediate veffel.

Defcription of the apparatus ufed in preparing the oxigenated muriatic acid in bleaching the pafte of paper for affignats. Defcription.

Fig. 1, Pl. VII. Plan of the apparatus. Plan.

1, 1, 1, &c. Eight furnaces, having a chimney of fheet iron common to each pair of furnaces.

2, 2, 2, &c. Eight veffels of caft iron, containing fand.

3, 3, 3, &c. Eight matraffes, balloons, or bottles of ftone ware, compact and well baked, intended to contain the materials which afford the gas. Each matrafs muft be filled only to one-third of its capacity at moft. Bodies of glafs of little thicknefs may alfo be ufed for this purpofe.

4, 4, 4, &c. Tubes of glafs to conduct the gas into the receiver. Or thefe tubes may be made of lead.

5. The

Plan. 5. The receiver. It is composed 1. of an external vessel, covered with plates of lead well soldered together, and provided near its bottom with a cock 6, to draw off the liquor when prepared. 2. Another vessel, 7, likewise covered with plates of lead within and without. This second tub is inverted in the first to contain the gas in proportion as it is disengaged, and to keep in contact with the water of the receiver, that portion of gas which had not time to be dissolved, in passing through that fluid.

There is a hole, 8, in the upper part of this second vessel. It serves to suffer the common air to escape when water is first poured into this receiver, and it is afterwards closed with a stopper of lead or cork, covered with paper, soaked in starch, and fastened to the cork by a piece of cloth or bladder, before the operation began.

Vertical section. Fig. 2. Vertical section of the apparatus.

Fig. 3. Elevation of the apparatus.

The dispotition of the furnaces about the receiver, and the circular form of the receiving vessels, was rendered necessary by the local circumstances of the laboratory in which our operations were carried on. In other circumstances square vessels might be employed, and all the furnaces might be ranged in a right line under a common chimney.

One thousand litres of water are placed in the receiver, holding in solution fifty kilograms of white purified and calcined potash.

When the disengagement of gas is effected by the muriatic acid, the materials are used in the following doses:

Oxide of manganese - - - - - - -	24 kilograms
Muriatic acid at 20 degrees of density according to the earometer of Baumé,	68
	—
	92

which makes for each of the eight distilling vessels 11½ kilograms of materials.

Charging and distillation. The operation is begun by charging the receiver with 1000 litres of alkaline water, after which the aperture 8 is closed with its stopper well luted. Each mattrass is then placed in its sand-bed; and pulverized manganese is introduced. The muriatic acid is poured upon the manganese, and the stoppers into which the tubes of communication pass, are duly placed. The juncture is luted with paper soaked in starch.

ftarch. And the lute is left to dry from fix to twelve hours, after which the fire is lighted in the furnaces.

The procefs of diftillation lafts from ten to twelve hours. When it is finifhed the tubes are unluted, the fire extinguifhed, and the mattrafes fuffered to cool in their fand beds, till the temperature of thefe beds has defcended to 60 or 70 degrees, (centigrade) at which period, water of the fame heat is poured into the mattrafes. The water dilutes the refidue of the diftillation, which mixture is to be poured out, and the veffels fuffered to cool in bafkets containing ftraw. If the precaution of introducing hot water in this manner upon the refidue were not taken, it would become fo folid when the operation is performed with fulphuric acid, in the manner we are about to defcribe, that it could not be extracted without much trouble and danger of breaking the veffels.

If the difengagement of the gas be made by fulphuric acid, the following dofes are ufed: Procefs with fulphuric acid.

Oxide of manganefe - - - -	25	kilograms
Muriate of foda - - - -	70	
Sulphuric acid at 50 degrees of denfity	25	
Total	120	

The acid is to be diluted with an equal bulk of water, or 16 litres, which will reduce its denfity to 31 degrees.

The eighth part of this for each mattrafs amounts to 14½ kilograms.

The oxide of manganefe and muriate of foda being pulverized are mixed together. The mattrafs is to be charged and the operation conducted as before defcribed. This method is the moft economical, becaufe the fulphuric acid is cheaper than the muriatic, and alfo becaufe it is practicable to obtain from the refidue of the diftillation, the foda of the muriate which is converted into fulphate of foda; that falt being decompofable by well known proceffes.

In order to meafure the force of thefe liquors, or their bleaching power, we made ufe of the folution of indigo prefcribed by Citizen Def roizilles, which is prepared in the following manner: To afcertain the ftrength of the bleaching liquor

In a glafs mattrafs put of concentrated fulphuric acid at 66 degrees of denfity, feven parts by weight, and of pulverized indigo one part; agitate the mixture, and plunge the ball of the By indigo.

the mattrafs to half its depth in water moderately heated, agitating it from time to time. The folution will be effected in two hours, after which it is to be diluted in 992 parts of water. This is the proof liquor. The ftronger the bleaching liquor, the greater number of parts of the folution of indigo it will render colourlefs, and we may by this proof determine the dofes of each kind of bleaching liquor to be employed, together with water, fo as to compofe a bath proper for immerfing the fubftance intended to be bleached.

One part by meafure of the bleaching liquid prepared as before mentioned, will ufually deftroy the blue colour of nine parts of the proof folution of indigo; it was of the fame ftrength as that of Javel, prepared by Citizen Alban.

Choice and Preparation of the Rags.

Selection of the materials for paper, quality of rags, &c. The ftrength or tenacity of paper depends upon the ftaple or fibre of the material from which it is made. Rags of new cloth and cordage compofe a paper more tough than old rags, and the firft of thefe materials prefents a great variety on account of the quality of the hemp or flax of which they are formed. Rags of fine new cloth, whether raw or bleached by the oxigenated muriatic acid, ftand in the firft rank, after which cordage and old rags may be claffed.

Bank or note paper. Paper intended for bills of exchange, or other commercial and legal inftruments ought to be tough, in order that it may not be eafily torn when thin, for this paper the materials of the firft clafs muft be intirely, or in large proportion employed. The price which the confumers are difpofed to pey for this article, is fufficient to indemnify the manufacturer for his care and induftry, as this kind of paper is fold in France for 5 or 6 francs the kilogram.

Common paper. The other papers alfo require to be more or lefs tough, according to their thinnefs, and the ufe to which they are applied, but a clear white colour is fought in paper of every defcription. The firft operation to which the rags are fubjected is forting, in order that each branch of the manufacture may have its appropriate material, after which they are cut with fhears into pieces of about one decimeter, or three or four inches fquare.

I will fuppofe that the object of the manufacturer is to obtain paper of a beautiful white. If it is intended to be thin, fo that,

that, for example, a ream of the fize denominated *raifin* fhould weigh only four or five kilograms, that is to fay, about one-third of the weight of common paper of the fame form. The manufacturer makes choice either of new rags already of a fine white, or of unbleached rags.

In the cafe of the white rags, it is fufficient to pafs them under the firft cylinder, then to give them a bath of the bleaching liquor, and afterwards a bath of fulphuric acid, as we fhall proceed to direct, after which they are paffed under the finifhing cylinder for feven or eight hours, and, laftly, conveyed to the working trough to be made into fheets of paper. Treatment of the rags, &c.

Rags, which have never been bleached, may be treated by either of the following proceffes, that is to fay, the firft, which preferves the utmoft degree of toughnefs to the paper, but is likewife the moft expenfive, confifts in decompofing the rag, and afterwards applying the method of Citizen Berthollet for bleaching piece goods; namely, fubjecting it to three or four lixiviations, and afterwards alternately to lixiviations, baths of the bleaching liquor, and baths of fulphuric acid. The weight of the raw unbleached material is diminifhed from 50 to 45 per cent. in thefe operations.

This method was the firft which we ufed for the affignat paper; but we foon perceived that we might omit moft of the lixiviations and baths of the bleaching fluid, and ftill preferve as much toughnefs as the paper required. Nothing further was neceffary for this purpofe than to fuffer the rag to undergo a degree of fermentation more or lefs advanced, by leaving it to rot. In this operation the colouring matter undergoes a flow combuftion, and paffes to a kind of faponaceous ftate, and is carried off by the water, by wafhing the rags in the veffel of the firft cylinder. Great advantage of fuffering the rags to undergo the fpontaneous change of rotting to a certain degree.

One fingle lixiviation, two baths of the bleaching liquor, and one of fulphuric acid, are then fufficient to bleach completely the raw rags or cordage. This is the fecond method. We were not, at that time, acquainted with the economical procefs of Citizen Chaptal in the operations of lixiviation. This will, no doubt, be ufed; but the effect of rotting, carefully conducted, will always be found very advantageous. The bleaching is greatly facilitated.

Laftly,

Laftly, if the rags be neither perfectly white nor raw and unbleached, but in a medium ftate, that they are left to rot for a fhorter time, for example, twelve or fourteen days, and are taken up when the heat of the fermentation raifes the thermometer to 30 or 35 degrees, after which the procefs is to be conducted as before mentioned.

Compofition of a bath of the bleaching liquor, for a pile of decompofed rags, weighing 50 kilograms.

Application and proportions of materials in the actual work.

For each heap of rags a certain number, for example, eight or nine, wooden tubs are difpofed in a line, capable of containing in the whole 600 litres of water: 450 litres of pure water is poured in, and 90 litres of bleaching liquor are added in equal portions to each of the veffels, after which the 50 kilograms of decompofed rags are difpofed in equal portions in each tub. The ftuff is left for about twelve hours in this bath, agitating it from time to time, after which it is to be completely wafhed in clean water, and put into a bath of fulphuric acid, compofed of water 200 litres, and acid at 50 degrees three kilograms, which bath will then have the ftrength of about four degrees of the earometer of Baumé.

The immerfion in the bath muft continue for three quarters of an hour or an hour, after which the materials muft be well wafhed in clear water, and carried to the mill to be manufactured.

If the action of the baths of bleaching liquor be not exhaufted by the immerfion of the decompofed rags (which may be afcertained by the folution of indigo), it may be applied to other materials of the fame kind.

Improvements.

Such was the ftate in which we left this new art in the year 3. Since that time Citizen Welter, to whom chemiftry and the arts are indebted for a number of ingenious proceffes, has fimplified that of preparing the bleaching liquor. He has found, for example, that inftead of the three veffels of the receiver, it is fufficient to employ two even for the fimple liquor that contains no fixed alkali.

Rife of the liquor without alkali.

It was before feen that we were obliged to employ an alkaline folution in the receiver, to prevent that odour which the fimple liquor emits when paper ftuff is agitated in the baths. The ufe of alkali anfwered our purpofe very well in this re-

fpect;

ſpect; but this expenditure, beſides weakening the bleaching liquor, nearly doubled our expence. Though this difference in the price was of little conſequence with regard to the object we then had in view, it is not ſo with regard to the common operation upon paper intended for ſale. Every means of economy muſt then be uſed. Now Citizen Welter found that it is eaſy to obviate the inconvenience of the ſimple liquor in the operation. His method conſiſts in no longer agitating the goods or material in an open bath, but to cloſe it exactly by means of a cover; and he agitates it by means of croſs pieces attached to a handle turned on the outſide.

A rough eſtimate of the price of the ſimple bleaching liquor prepared by the ſulphuric acid, this being the moſt economical proceſs. Eſtimate of charges.

The receiver is ſuppoſed to contain 1000 litres of water.

25 kilograms of oxide of manganeſe coſt at moſt - - -	15fr.	0c.
70 kilograms of muriate of ſoda -	7fr.	0c.
25 kilograms of ſulphuric acid, at 50 deg.	37fr.	50c.
Three days work, principal men -	9fr.	0c.
Three days ——, aſſiſtant or labourer	4fr.	50c.
Fuel, about - - -	3fr.	
Wear and tear - - -	6fr.	
Our apparatus coſt 622 franks, and the carriage and fixing increaſed our expence to 1000 franks, the intereſt of which, at 10 per cent. is 100 franks; and if the work be repeated ſo many times in the year, the intereſt per operation will be - - -	1fr.	
	83fr.	

Hence the litre of bleaching liquor will coſt nearly 9 cents in round numbers. *

* As the price of all theſe ſeveral items in France muſt materially differ from the ſame in England, I have thought it unneceſſary to reduce the numbers.

Eſtimate

Estimate of the increase of expence occasioned in the operation upon a pile of 50 kilograms of the paste of paper, supposing one bath of the bleaching liquor and one of sulphuric acid, which is most commonly the case.

Ninety litres of the bleaching liquor at nine cents.		9c.
	8fr.	10c.
Three kilograms of sulphuric acid, at 1fr. 50c. - - -	4fr.	50c.
Workmanship - - -		50c.
Total	13fr.	10c.

Which gives for each kilogram of paper an expence of 0,262 franks, or about 27 cents. Now the common paper in the market usually sells for about 1 fr. 30c. or 1fr. 40c. the kilogram, and with the simple augmentation of 27 cents for the operations of bleaching, it obtains the preference beyond that which is sold for three, four, or even five franks, which can only be obtuined in limited quantity, on account of the selection of rags. The foregoing methods must therefore produce a great diminution in the price of fine paper. They are more particularly advantageous when applied to the manufacture of thin paper, because the expences of bleaching are always proportioned to the weight of the material, and consequently are least upon thin paper.

Distilling apparatus of *Athenas.*

I shall conclude this memoir with a description of an apparatus, invented by Citizen Athenas, for preparing the oxigenated muriatic acid. He had the complaisance to lend it to us. This apparatus is remarkable for its simplicity, as it requires no vessels of earth or glass, nor intermediate vessels, nor tubes of communication.

Fig. 4, Plate VII. represents the horizontal section, and Fig. 5 the vertical section. The description of this last will be sufficient to explain the construction.

9 represents the ash-hole.

10 the fire-place, having its chimney turned on one side.

11. An iron boiler, containing water kept at the heat of ebullition, serving as a bath for the cucurbit, and effecting the disengagement of the gas.

12. The distilling vessel of lead, in which the subject matters of the operation are put.

13. A

13. A veffel lined with lead, interfected by the veffel 12, with which it is foldered.

14. A cock to draw off the fluid when prepared.

15. A receiver of lead inverted in the tub. A hole, 16, may be made in this receiver to let out the atmofpheric air when it is plunged in the tub, which hole muft be clofed before the difengagement of the gas begins. The inverted receiver bears upon three projections of wood, which keep it at a little diftance above the bottom of the tub.

When the ftrength of the liquor obtained by this apparatus was compared with ours, it was found that with equal proportions of materials and of water its difcolouring action upon indigo was no more than about the half of ours. Confequently there is a real lofs in the confumption of thefe articles. This muft be attributed to the low degree of heat, which does not expel the whole of the gas. I have neverthelefs judged that it would be of ufe to defcribe this ingenious mechanifm, the fimplicity of which will, no doubt, give fatisfaction to artifts. It is obvioufly capable of improvement, either by placing on one fide a feparate furnace, with a fand and ordinary diftilling veffel, whence the gas may be conducted beneath the receiver by a leaden tube; or, according to the actual difpofition of the apparatus, a liquid or dry bath may be fubftituted to that of water, fo as to communicate a higher degree of heat, provided the temperature be lefs than will melt lead, a condition which admits the defired extrication of the gas.

This laft apparatus was lefs effective than the former.

But it may be improved by raifing the heat, and is very fimple.

IX.

On the Sound produced by a Current of Hidrogen Gas paffing through a Tube. With a Letter from Dr.* HIGGINS, *refpecting the Time of its Difcovery.*

INTO a glafs bottle is put fulphuric acid and iron filings, through the cork of which a glafs tube is paffed; the upper extremity is capillary: then by fetting fire to the hidrogen gas which efcapes by this extremity, a continued current or jet

The flame of hidrogen paffing through a capillary aperture, under a tube,

* Bulletin des Sciences, No. 56, An. 10.

of flame is produced, which is let pass into a tube of either glass, metal, earth, or any other substance; when the following phenomena are observed.

luces clear ıds.

If the tube be not too large, the flame becomes smaller as it is depressed, and when it is much reduced, the tube emits very clear sounds.

But if the tube be too narrow, the flame will be extinguished, and in proportion as the tube is larger, the sound diminishes; so that there is a certain limit at which it totally ceases, which also happens when the tube is too long.

The sounds may be varied at pleasure, by either using tubes of different figures and dimensions, or made of various substances.

is experiment, le in Italy;

These experiments have been made in Italy. Citizen Brugnatelli had described them in his Annals of Chemistry; and he has repeated them with Citizen Volta in the cabinet of the Polytechnic School, in the presence of several persons.

Letter *from* Dr. Higgins.

To Mr. NICHOLSON.

Dear Sir,

ras made 25 ago by Dr. ins.

IT was in the year 1777, that I first exhibited the experiment you mention, and produced several sweet tones, according to the width, length, and thickness of the glass jar or sealed tube, held by the bottom, inverted to some depth over the flame of hidrogen gas. This experiment was afterwards shewn by some of my pupils, to amuse their friends, and particularly by my cousin, Mr. Higgins, who now teaches in Dublin; who exhibited it at Sir Joseph Banks's, and, if I am not misinformed, at Oxford, where he served the professor as operator. *

y of the ery.

These tones first occurred unexpectedly, whilst I was making this easy and popular experiment, to shew the water formed during the slow combustion of the slender stream of hidrogen gas in the air of the jar. And as I always had a

* I remember the experiment at Mr. Kirwan's, in 1784, which led me to request the information here given.

great

great variety of ſuch jars on the table, many different tones were produced on the very firſt day, by applying jars of different capacity and thickneſs.

I am, SIR,

Your's reſpectfully,

B. HIGGINS.

Percy Hotel, Jan. 12, 1802.

Speculation on its cauſe.

The cauſe of the curious phenomenon here deſcribed ſeems difficult to be clearly aſcertained. As it is univerſally admitted that ſound is cauſed by undulations, that is to ſay, alternate condenſations and rarefactions of the air, or ſome more ſubtle fluid, we are naturally led to an intenſion and remiſſion of the flame of the hidrogen. It is probable that all flame undulates. The flame of a candle is ſaid to dance when this effect is moſt remarkably evident. I ſuppoſe the ſupply of oily vapor enlarges the flame, until its ſurface becomes ſo extenſive, as to abſorb oxigen in greater quantity than the fuel can be ſupplied to keep up a combuſtion of that ſize: it therefore becomes leſs, until by the diminution of ſurface, the due proportion of oily vapor, compared with the oxigen, again exceeds. And in this way we may form a notion of perpetual undulations of ſize. If theſe changes were rapid and ſtrong enough, the air would be ſtruck by them into ſonorous undulations. In the ſame manner perhaps we may account for the loud chirping noiſe I have obſerved to be produced by the inflamed drops of tallow which fall from a candle held nearly upſide down at five or ſix feet from the ground.

Cauſe why the flame of a candle dances.

A chirping noiſe produced by flame.

W. N.

X.

Description of the Graphometer, or Instrument of CIT. CARANGEAU, *for measuring the Angles of Crystals.—From the Minerulogie of* CIT. HAUY *.

Structure of the graphometer. Two concentric, equal quadrantal arcs joined by an hinge; to which are applied compass legs or radii, capable of being shortened to apply to small crystals, &c.

IN order to determine the mutual inclination of the faces of a crystal, or its prominent angles, an instrument is employed, for the invention of which we are indebted to Citizen Carangeau. This instrument, which considerably resembles the graphometer, consists of a semicircle M T N, (Pl. VIII. fig. 1.) of brass or silver, divided into degrees, to which two indexes, A B and F G, are applied, the one of which, F G, is hollowed out from *u* to R, from face to face, except at the place K, where a small portion is left for the purpose of giving greater strength to the instrument. This index is attached at R and *c* to a brass piece or rule situated behind, and forming the same piece with the limb. The connection of the index with this rule is effected by means of two small milled-head-screws, inserted in the groove. The other index, A B, is hollowed out in the same manner from *x* to *c*, where it is attached above the former, by means of the screw at the center, which traverses the two grooves. By loosening the screws, we may shorten at pleasure the parts *c* G and *c* B of the two indexes, accordingly as circumstances may require.

The index A B having only one point of attachment at *c*, at the center of the circle, moves round this circle, whilst the index G F remains constantly in the direction of the diameter, which passes through the points zero and 180 degrees.

It may be proper to remark, that the upper part of the index A B ought to be sloped to an edge, on its side *s z*, the direction of which edge, if prolonged, would pass through the center *c* of the instrument. The reason is, that this side is the fiducial edge; that is to say, it indicates upon the graduated circumference the dimensions of the angle we wish to measure.

Mode of operation.

Let us now suppose that we are desirous of measuring in any crystal, the angle which two contiguous planes form with

* This ingenious instrument has been in use a considerable time, and deserves to be more generally known. W. N.

each

each other. It is known that this angle is equal to that of two lines drawn to any single point of the edge which unites these planes, provided they be perpendicular with this edge, and situated in the planes themselves. To find this angle, we dispose the instrument in such a manner, that the portions *c* G and *c* B of the two indexes shall have no space between themselves and the points in question, and that their sides shall be perpendicular to the edge at which they join. In this case, the faces which embrace the crystal are tangents to the two planes whose incidence we wish to measure. This being done, we look on the circumference of the instrument for the degree marked by the fiducial edge *s z*, or the angle which this line forms with that which passes through the center *c* and the point zero, which angle is equal to that formed by the two portions G *c* and *c* B of the indexes, because vertically opposite.

It is an advantage to be able to shorten these parts at pleasure, in order to avoid the obstacles which would otherwise render the operation impracticable, either from the gangue to which the crystal adheres, or from the contiguous crystals by which it is partly surrounded.

Advantage of shortening the legs.

Cases, however, occur in which this expedient would not be sufficient, and we should find ourselves embarrassed by the part of the semicircle situated towards M, if its position were invariable. The ingenious inventor of the instrument has guarded against this inconvenience by means of the following mechanism:

In some crystals this expedient not sufficient;

The screw situated at *c* holds not only the two indexes, but also a steel radius placed below the brass arm, upon which the index G F is immediately applied. The upper extremity of this radius, or that which is situated towards O, has an excavation into which a brass stem enters, which is likewise provided with a screw. The semicircle is besides divided into two pieces, at the place of the 90th degree; so that, by means of a hinge at that part, the quadrant T M may be folded behind the quadrant T N, and is, as it were, suppressed. When we wish to perform this movement, we loosen the screw which supported the upper part of the radius *c* O, disengage the excavation which terminates this radius from the screw which was inserted into it, and draw back the radius till it lies beneath the brass arm which supports the index G F.

but part of the circular limb must be occasionally removed.

When

When the angle which we meafure exceeds 90 degrees, we return the quadrantal arc T M to its former fituation, in order to afcertain its dimenfions.

The precife meafure of angles completes the defcription of a cryftal.

The utility of the goniometer will be very apparent to every one who confiders how interefting it is in defcriptions of cryftals, to know the angles which their faces form with each other. Thefe indications render the defcription appropriate, by diftinctive and truly characteriftic traits: without them it can be nothing more than a rude and imperfect fketch, that may apply to a variety of different objects.

Without this they cannot be diftinguifhed.

Inftances.

Thus we do not give a determinate notion of the dodecahedral zircon, when we merely fay that it is a prifm of four fides, terminated by fummits with four rhombufes attached to the longitudinal edges. This defcription applies equally to the Harmotome (cruciform hyacinth) to the ftilbite, to oxided tin, &c.; but if we add, that the fides form right angles with each other, and that the faces of the fummit are inclined towards each other in an angle of 124° 12′, we confine the defcription to the zircon. If we fay that the inclination is 121° 57′, we defcribe the harmotome; if we fay that there are two different inclinations, the one of 123° 32′, and the other of 112° 14′, we defcribe the ftilbite.

Other cafes.

And ftill farther: Several varieties of the fame fubftance may prefent fimilar forms, differing only in the meafures of their angles. Such are, on the one hand, the fix rhomboides, and on the other the two decahedrons with rhombic faces, which are found in the carbonates of lime. How fhall we defcribe with accuracy all thofe varieties, which differ only in the more or lefs, unlefs we give the differences with precifion? There are even cafes in which the ufe of the goniometer is the only means of avoiding an error, which otherwife would not fail to infinuate itfelf into the defcription. Thus the calcareous rhomboid, the angles of which differ only by about 2° 18′ from the right angle, was at firft taken for a cube, and we fhould have continued to denominate it cubic calcareous fpar, if geometrical meafurement had not rectified this denomination, which is faulty in two refpects, both in itfelf, and with reference to the theory, which demonftrates that the exiftence of the cube cannot in this inftance be reconciled with any of the fymetric laws of diminution.

XI.—Letter

XI.

Letter of Professor VOLTA *to* J. C. DELAMETHRIE, *on the Galvanic Phenomena.*

SIR, *Paris,* 18 *Vendemiaire, Year* 10.

YOU have requested me to give you an account of the experiments by which I demonstrate, in a convincing manner, what I have always maintained, namely, that the pretended agent, or *galvanic fluid*, is nothing but common electrical *fluid*, and that this fluid is incited and moved by the simple *mutual contact of different conductors*, particularly the metallic; shewing that two metals of different kinds, connected together, produce already a small quantity of true electricity, the force and kind of which I have determined; that the effects of my new apparatus (which might be termed electromotors), whether consisting of a pile, or in a row of glasses, which have so much excited the attention of philosophers, chemists, and physicians; that these so powerful and marvellous effects are absolutely no more than the sum total of the effects of a series of several similar metallic couples or pairs; and that the chemical phenomena themselves, which are obtained by them, of the decomposition of water and other liquids, the oxidation of metals, &c. are secondary effects; effects, I mean, of this electricity, of this continual current of electrical fluid, which, by the above-mentioned action of the connected metals, establishes itself as soon as we form a communication between the two extremities of the apparatus, by means of a conducting bow; and when once established, maintains itself, and continues as long as the circuit remains interrupted. You have requested this sketch from me, to be inserted in the next number of your Journal de Physique, convinced in your own mind of the truth of these observations by some of the experiments which I had the pleasure of shewing you yesterday with my small portable apparatus, in presence of the celebrated philosopher of Geneva, M. Pictet, and some other friends. I regret that I have not sufficient time to enlarge the essay which I send you, so as in some measure to comply with your invitation, and which can answer your expectations only in a partial manner. Accept it then as the precursor of a more ample memoir, which I intend shortly to publish.

General outline of the doctrine of the author respecting galvanism.

That the electrical effects are the primary causes of the oxidation, &c. &c.

I

Narrative of experiments.

I began by shewing you, by means of experiments, delicate indeed but yet simple, that phenomena unequivocally electrical are produced by the mere contact of two different metals, without the intervention of any wet substance—experiments which ought to be considered as fundamental.

Electrometer and condensers.

In order to render this electricity (which is so feeble that without other artifices it would remain imperceptible) sensible and manifest, I employ my electrometers with fine straws, combined with my condensers, the best of which are those made with two metallic discs, which apply very exactly to each other by their faces, which are very flat, and covered with a slight layer of sealing wax, or, which is better, of good lac varnish.

Insulated discs of copper and zinc are applied together.

The first method of performing this experiment was to take two other discs or plates, the one of copper and the other of zinc; to hold each by an insulating handle (of glass covered with sealing wax); to apply them for an instant to each other by their flat faces, and afterwards separating them dextrously, to bring them into contact with the electrometer, which then indicated, by the divergence of its straws, to the distance of about a line from each other, the electricity which each of the plates had acquired, and whether the electricity was positive (or el. +) in the zinc, and negative (or el. −) in the copper, as could be shewn by approaching a stick of sealing wax, that had been rubbed, to the same electrometer.

On the separation, the zinc was el. +, and the copper el. −.

These plates not only *move* the electricity, but *condense* it.

It is proper to observe in this experiment, that the two plates, at the same time that they are *motors of electricity* by virtue of their mutual contact, as different metals, perform also the function of condensers, because they are presented the one to the other by a large surface, in consequence of which their contrary electricities are counterbalanced in the best possible manner. This is the reason why this positive electricity in the plate of zinc, and negative in that of copper, which otherwise would not rise to more than about a sixteenth of a degree, and which in fact does not rise higher as long as these same plates remain applied the one to the other, elevates itself, on detaching them, to 1, 1½, or 2 degrees, and even more.

To increase the electric tension, the discs, where separated, are

This degree of electricity may appear trifling; it does not satisfy some persons, who always wish to see effects exemplified upon a large scale. Be it so. In order to obtain electri-

cal phenomena much more distinctly marked, I generally employ a second condenser, mounted upon the electrometer itself, and proceed in the following manner:—I apply the plates of copper and of zinc to each other, and separate them several times, at each separation bringing one of these insulated plates into contact with the upper disc of the condenser, and the other, also insulated, with the lower disc, which is attached to the electrometer. When this contact has been repeated ten, twelve, or twenty times, the disc of the above-mentioned condenser being raised, the electrometer supports the inferior disc alone, which elevates itself to 10, 12, 15, 20 degrees, &c. &c.

repeatedly applied to another condenser.

It might be imagined that, independent of the action of the condenser, the extent of contact between the two different metals greatly contributes, as such, to raise the electricity to the degree which we have seen, and that we should obtain a much inferior degree, if they touched each other only by a few points. But I prove the contrary; that is to say, that in the one case, as well as in the other, the electric tension rises, during the contact, to the same point, which is about a sixtieth part of a degree of my electrometers of thin straws, when the metals are zinc and copper, and a little more when they are zinc and silver; which tension requiring a quantity of electric fluid in the plate which performs the office of condenser, proportionably larger, accordingly as it condenses 60, 150, 200 times, it is evident why we obtain 1, 1½, 2 degrees, &c. &c.

The application of the flat surfaces of the zinc and copper does not increase their motive power.

In order to prove that the contact of two metals of little extent, and which even subsists only at some points, displaces the electric fluid in such a manner as to raise the tension in these metals to the same degree; I join a small plate of copper with another of zinc, either similar or dissimilar with respect to figure and size, applying them to each other at a few points only, or at more points, or even soldering them together end to end. See Plate VIII. fig. 3.

Metals of other forms brought into contact by smaller or minute surfaces.

Holding the piece z of zinc with two fingers, or in any other manner, I make the other, c, of copper, communicate with the superior disc of the condenser, whilst the inferior disc communicates, as it ought, with the ground. A moment afterwards, raising this upper disc into the air, and holding it insulated, it gives me by the electrometer from 2 to 3 degrees of

Manipulation. A single couple of pieces, one of which is held in the hand, and the other applied to the condenser, produces

electricity; equal to that of the larger contacts;

of negative electricity (el. —), accordingly, as such a condenser condenses from 120 to 180 times. This proves that the electric tension of the above-mentioned plate *c* was about a sixtieth of a degree, or nearly equal to that which the two plates of copper and of zinc acquired in the preceding experiments, when applied to each other by the whole extent of their flat surfaces.

which is reversed by touching with the other metal.

When we reverse the experiment, that is to say, when we cause the plate *z* of zinc to communicate with the condenser, we likewise obtain from two to three degrees, but of positive electricity (el. +.)

If the zinc piece touch a copper condenser while the other piece (of copper) is held in the hand, no effect follows;

However, if the disc of the condenser be copper, and the plate *z* touch it immediately without any intervening substance, we obtain nothing, because the zinc being then in contact, at the two opposite ends, with copper and copper, two equal forces act in opposite directions, and by that means destroy or counterbalance each other.

unless moisture be interposed.

It is therefore necessary that the communication of the plate of zinc *z* with the copper disc of the condenser should be effected by the interposition of a conductor, which should be a simple conductor nearly, an humid conductor, such as a piece of wet card or cloth.

But it is asserted, that the humidity acts merely as a conductor, to prevent the opposite metallic contacts in the pile.

As to the rest, the action which excites and gives motion to the electric fluid does not exert itself, as has been erroneously thought, at the contact of the wet substance with the metal, where it exerts so very small an action, that it may be disregarded in comparison with that which takes place, as all my experiments prove, at the place of contact of different metals with each other. Consequently the true element of my electromotive apparatus, of the pile, of cups, and others that may be constructed according to the same principles, is the simple metallic couple, or pair, composed of two different metals, and not a moist substance applied to a metallic one, or inclosed between two different metals, as most philosophers have pretended. The humid strata employed in these complicated apparatus are applied therefore for no other purpose than to effect a mutual communication between all the metallic pairs, each to each, ranged in such a manner as to impel the electric fluid in one direction, or in order to make them communicate, so that there may be no action in a direction contrary to the others.

After

After having well afcertained the degree of electricity which I obtain from one fingle pair of thefe metallic pieces, as fhewn by the condenfer which I employ, I proceed to fhew, that with two, three, four pairs, &c. properly arranged, that is to fay, difpofed all in the fame direction, and communicating the one with the other, by as many humid ftrata (which are neceffary, in order that there may be no contrary actions, as I have already fhewn), we have exactly the double, triple, quadruple, &c. fo that if with a fingle pair we fhould be able to electrify the condenfer to that point as to make it indicate, by the electrometer, three degrees, for example, we fhould obtain fix with two, nine with three, twelve with four, &c. if not exactly, yet nearly fo. You have feen thefe experiments, and have appeared to be very well fatisfied with them, as well as Mr. Pictet, who feemed enchanted with them, and was never tired with feeing them repeated.

The intenfity of a fucceffion of couples is the fame multiple of that of a fingle couple of plates.

Here then we have already conftructed a fmall pile, which however does not yet afford any indications, by the electrometer without the aid of the condenfer. In order that it may immediately afford thefe, in order that it may arrive at one whole degree of electric tenfion, which it will fcarcely be poffible to diftinguifh, becaufe it amounts to no more than half a line on the electrometer, it is neceffary that fuch a pile fhould be compofed of about fixty of thefe combinations of copper and zinc, or, which is better, of filver and zinc, on account of the fixtieth of a degree which each combination yields, as I have already remarked. It then alfo affords fhocks if we touch its two extremities with wet fingers, and much ftronger fhocks if we touch them with metals grafped by large furfaces in the hands well moiftened, by which means a much better communication is eftablifhed.

This is the pile; and gives fhocks.

In this manner we may receive fhocks from an apparatus, whether in pile or with cups, of 30 and even of 20 pairs, provided the metals be fufficiently clean, and efpecially if the humid ftrata be moiftened, not with mere pure water, but with a confiderably ftrong faline folution.

Saline liquids, inftead of water, increafe the effect;

Thefe faline liquids, however, do not properly augment the electric force;—not at all;—they merely facilitate the paffage, and leave a freer courfe for the electric fluid, being much better conductors than fimple water, as feveral other experiments prove.

but merely, *it is afferted*, becaufe they are better conductors.

In

Experiment offered in proof. A series of glasses, with the metals and water, give a certain electric intensity and shock; but on adding salt to the water, the intensity is the same, though the shock is much greater.

In order to establish this fact, and render it evident to such as have found a difficulty in admitting it, that the electric force or tension is, if not entirely, yet very nearly the same, whether the wet layers be moistened with pure water or with a saline solution, though the difference in the shock is so great; I have frequently made the following experiments, of which I have spoken to you, and which I would have shewn to you, if I had been provided with the requisite articles. I take 30 cups or drinking glasses, with which I construct one of these apparatuses which I term a crown of cups, putting into them a sufficient quantity of pure water, and causing them to communicate, the first with the second, the second with the third, and so on successively to the last, by means of metallic wires, which terminate at one end in a plate of copper, and at the other in one of zinc, and are all turned in the same direction. The apparatus being constructed in this manner, I try its electric force, by causing the first of the cups to communicate with the ground, and applying the condenser to a piece of metal which is partly immersed in the last: this condenser, when I afterwards withdraw it, and separate one of its discs from the other in the proper manner, and without delay, gives me 40, 60 degrees, and more, according to its condensing force. I also try the shock in the most advantageous manner, and find that it is very slight: after having well ascertained both the degree of the electricity and the weakness of the shock, I add a pinch of salt to each cup, and repeating the proofs, I find that the electricity has not been at all increased, the condenser giving me still only 40 or 60 degrees, as before; but the shocks are incomparably stronger.

The apparatus charges a large battery almost instantaneously.

There are many other experiments, which I have described to you verbally, and which I would willingly have performed before you, had I not been in want of the requisite apparatus. I informed you—at which you were much astonished, and Mr. Pictet still more so—that with one apparatus I charge a Leyden phial, whatever its capacity may be, and even a large battery; that I charge them in an instant, or, to speak more accurately, in less than a twentieth part of a second, and almost to the same degree as the apparatus itself, namely, to about one degree of tension, if the apparatus be composed of 60 pairs; to two degrees if it contain 120, &c.;—that I am then able to draw, by the help of the condenser, a strong spark from

from a fmall jar charged in this manner, a great number of fimilar fparks from a large jar, and almoft without limit from batteries, as I am actually able to draw them without limit of number from the apparatus itfelf.

I have informed you, that large bottles charged in this manner gave me moderate fhocks, and batteries pretty ftrong ones, as high as the elbow, and higher; that the fhocks of a battery of 10 fquare feet of coating, and charged in lefs than a twentieth part of a fecond by one of my apparatufes of 200 metallic pairs, are very violent and almoft infupportable; for I have not yet made any trial with larger batteries; but that there is every probability that the violence of the fhocks increafes with the fize of the batteries, as far as a certain term, which I am not able to define; fo that it would be poffible, with batteries of 40, 60, and 100 fquare feet, to give confiderably ftrong fhocks, by charging them by the tranfient contact of a pile only 60, 40, 30, or ftill fewer pairs.

Strong fhocks from batteries fo charged.

I have explained to you the manner in which one ought to proceed in order to perform thefe experiments with fuccefs; that it is particularly neceffary we fhould carefully avoid the flighteft interruptions in the communications of the conductors with the coatings of the bottles, and between each other, and that this becomes ftill more neceffary when the electromotive apparatus, being compofed of a fmall number of pairs, poffeffes but little power, fo as to be unable to overcome the flighteft obftacle that might oppofe the paffage and the courfe of the electric fluid.

Cautions in order to fucceed in charging a battery by galvanifm.

Laftly, I remarked to you, that thefe experiments confirm in a very evident manner what all the others already fuggefted, namely, that the quantity of electric fluid fet in motion by my apparatus is much larger for every moment of time than that which is fet in motion by the ordinary electrical machines; that the former afford it in much greater abundance than the latter, when the object is to produce, not an accumulation of electric fluid in infulated bodies, in order to raife the electricity to a high degree of tenfion, which may be done with thofe machines, but by no means with piles and other fimilar apparatufes, unlefs we alfo employ condenfers; but where we require a conftant current of this fluid, fupported by a continued action of a circle of condenfers not infulated; nay, one of my apparatufes, of only 60 or 30 metallic pairs, pours out

The pile moves a much greater quantity of electricity than any machine.

in every inſtant, or in any given time, more electric fluid, if it meets with no obſtacle, or if the fluid be not obſtructed by the too ſmall capacity of the recipient into which it is infuſed, than one of the moſt active electrical machines with cylinders or plates of glaſs. In fact, where ſhall we find a machine capable of charging a very large battery to one or even half a degree in leſs than an eighth part of a ſecond, of pouring into it a ſufficient quantity of electric fluid to enable us afterwards to draw from it, by the condenſer, a great number of ſparks in ſucceſſion, as is done by one of the above-mentioned apparatuſes? *

Mention of other experiments.

The other experiments, which I was able to ſhew you in part, relate to the different electroſcopic phenomena which the apparatus preſents, accordingly as the one or the other of its extremities communicates with the ground, or both of them, or neither one nor the other, or as they communicate only between themſelves and with the ground at the ſame time; accordingly as theſe communications are effected by perfect conductors, conductors more or leſs imperfect, &c. all which circumſtances ſingularly modify and produce great variations in the reſults, which often appear curious and even anomalous, but which, nevertheleſs, I think myſelf able to explain in a ſatisfactory manner, without deviating from my principles and ſound electrical theory, attention being paid to the mode in which the imperfect or bad conductors act. It would carry me too far to enter at preſent into theſe details; what you have already ſeen, and what I have communicated, may be ſufficient for the preſent occaſion.

Obſervations on the preceding Memoir. W. N.

Theory of Volta too haſtily adopted.

Sig. Volta, and many of our philoſophical neighbours in France, will, no doubt, think, on re-conſideration of the facts, that they have been too precipitate in admitting the electric energy as the only effective agent in the phenomena of the pile, and that the fluids act merely as conductors. The late interruption to correſpondence may probably have rendered the contents of my late Journal little known in France, otherwiſe the learned author of this memoir would

* On this ſubject ſee Philoſoph. Journal, 4to, IV. 243—245. N.

have met a fatal objection to that part of his theory, which gives all to the metals and nothing to the fluids, in * Mr. Davy's galvanic pile, consisting of one metal throughout, but with different strata of fluids: for example thus; metal, cloth soaked in dilute nitrous acid, cloth soaked in water, cloth soaked in sulphuret of potash;—then the same metal, and nitrous acid, and water, and sulphuret;—the metal, &c. Or if a trough be used, the separation between the acid and sulphuret may be made by a plate of horn, and the two fluids may be connected by a slip of wetted paper hung over the edge of the horn, which will not cause the fluids to mix, because water is lighter than either. The metals, separately and successfully tried, were silver, copper, zinc, and lead.

Davy's galvanic pile with one kind of metal only and three fluids.

Trough of the same construction.

To this I will here add, from conversation, an experiment of the same philosopher, which is no less conclusive as to the direct efficacy of the fluid in this apparatus; because the same electric power is made to move either from the top or bottom of a pile of two metals, according to the nature of the interposed fluid.—If a pile of copper and iron be constructed as usual, with water interposed, the iron becomes electrified *plus* and the copper *minus*; but if the same, or a similar pile, be constructed with no other difference than that sulphuret of potash is used instead of water, the iron is electrified *minus* and the copper *plus*. In the first case the iron is oxided; but in the second there is no oxidation of this metal, and the copper is oxided, and probably sulphurated.

Electric current of the common pile reversed by changing the fluid.

Lastly, we have another instance of the power and importance of the fluids in the article which follows the present, where charcoal alone is used, and the leading condition is, that two different fluids shall be used.

Galvanic series of charcoal only and two fluids.

As we know by the experiments of De Saussure, and many others, that chemical changes do disturb the equilibrium of electricity, and they certainly take place in the pile, it seems at least probable that these may have the chief agency in the apparatus. With regard to the principle of electric motors of Sig. Volta, I must observe, that Bennet made many direct experiments of applying different metals, by the single

Chemical changes are known to produce electricity; and as such changes happen in the pile, they may probably cause its electricity.

* See notice in the Scientific News, Phil. Journal, 4to. for May, 1801, Vol. V. p. 78; and the subject is fully treated in the Philos. Trans. for 1801, in a paper inserted in the same volume of our Journal, 4to. p. 341.

and

Bennet made experiments on electricity by contact of metals before A. D. 1789.—Cavallo before 1795.

and double touch, to the plates of the doubler, followed by the production of electricity, by him called adhesive electricity, which were published in his *New Experiments on Electricity* in 1789, pages 86 to 102; and others were made by Cavallo o the electricity produced by the contact or stroke of a piece a metal, let fall out of the hand, for the most part, upon an insulated metallic plate, which were published in the third volume of his *Electricity* in 1795. I do not know the date of Volta's experiments, but believe them to be much later than those of the same kind by Bennet. This last philosopher, as well as Cavallo, appears to think that different bodies have different attractions or capacities for electricity; but the singular hypothesis of electromotion, or a perpetual current of electricity being produced, by the contact of two different metals is, I apprehend, peculiar to Volta.

They supposed various electric capacities; but the hypothesis of a current of electricity produced by metallic contacts belongs to Volta.

XII.

An Account of a Method of Constructing Simple and Compound Galvanic Combinations, without the Use of Metallic Substances, by means of Charcoal and different Fluids. By Mr. DAVY.

Charcoal between nitric acid and water affords galvanic effect.

1. IF a piece of well burned charcoal be brought in contact at one of its surfaces with a portion of water, and at another surface with a portion of nitric acid, a simple galvanic combination will be formed when the two fluids are connected together. And the powers of it are demonstrated by its agency upon the limbs of frogs, and by its effects upon the organs of sense.

Series of such combinations.

2. A compound galvanic combination or a galvanic battery may be formed from a number of series composed of the same substances: but in this case the fluid elements of each series, not being immediately in contact, must be connected with similar elements in other series in an order of regular alternation, such as water, charcoal, acid; water, charcoal, acid; and so on.

Charcoal battery by a series of glasses alternately charged with nitric acid and water, and al-

3. The best mode that has yet occurred of constructing galvanic batteries with charcoal, is by means of a number of glasses, which are made to contain, alternately, nitrous acid, and water, and which are connected in pairs by means of moistened

fened cloth. The pieces of charcoal ufed are made from very denfe wood, fuch as box, or lignum vitæ; and in this cafe the fluids will not penetrate into them by capillary attraction, much beyond the places of their primary contact. Their forms are thofe of arcs, fo that each piece connects together two glaffes; but in inftances where fingle pieces of charcoal cannot be obtained of the proper fhape, two long and thin flips may be faftened together by filk, fo as to form the angle neceffary to their infertion into the glaffes. ternately connected by charcoal and flips of wet cloth.

4. Twenty feries in a battery of this kind produce fenfible but feeble fhocks, and when a fingle metallic feries with a gold wire and two glaffes of water is fubftituted for one of the primary feries, hidrogen is given out by the metallic point in the glafs of water in the place of the acid, whilft oxigen is evolved from the point in the other glafs. Its power.

5. In the galvanic batteries with charcoal, fulphuric acid may be fubftituted for nitric acid; and folution of fulphuret of potafh for the water, without any material alteration in the nature of the agency; the folution of the fulphuret indeed, feems, in fome meafure, to increafe its intenfity, and combinations containing this fubftance, denfe charcoal, and concentrated nitric acid, appear to be fuperior in activity to fimilar combinations containing copper and the fame fluid elements, and to be nearly equal to thofe compofed of zinc, filver, and water. Other fluids.

January 9, 1802.

XIII.

Short Analyfis of the Principles and Structure of Mr. CLOSE'S *Hydraulic Engine.*—W. N.

AN eminent engineer, to whofe talents and invention fociety is highly indebted, writes to me that he cannot underftand Mr. Clofe's engines, and requefts fome explanation, as he does not conceive that impoffibilities would have my fanction. Though I am convinced that an attentive revifal of the defcriptions would render my elucidation unneceffary, yet as that which he requefts may not be unacceptable to other readers, I have here annexed a few obfervations; viz. An explanation of Mr. *Clofe's* engines requefted.

1. That

Short enumeration of its principles and mode of operation.

1. That the water (of which part is intended to be raifed above the level) muft have a fall. 2. That this fall affords the ufual difference between the legs of the fyphon. 3. That if the fyphon had two or more afcending legs, water would rife through them all. 4. That if one of thefe legs were fmall and contained air, that air would pafs into the great fyphon, and if the velocity of the fluid were rapid enough, the bubbles would pafs down along with the water, inftead of rifing to the top and breaking the column. 5. That if there were a contrivance of two cocks in the water way of the fmall afcending leg, and both were fhut, the portion of water might be drawn out from between them by a fide cock, and that fpace filled with air. 6. That this fpace may, if thought fit, be a globe or other veffel. 7. That this fpace being above the level, the water thus drawn off *is in fact raifed.* 8. That by fhutting the fide cock, and opening the two others, the included air would afcend and be carried down the great fyphon. 9. After which another like portion of water might be drawn out as before, &c. &c. And, 10. It is clear that by fmall fyphon work the cocks might be opened and fhut without attendance.

The water is raifed as ufual by its fall.

This is only one of the many methods of raifing water above its level by virtue of its fall. It certainly poffeffes merit and originality of invention; but whether it be generally preferable to other known methods is another queftion. For my part, I think it is of advantage to our general ftock of knowledge and invention, to publifh all ingenious novelties, without any particular folicitude refpecting the extent of their power. A variety of circumftances and local fituations afford opportunities of applying engines, or their parts, with fingular advantage, which in the general contemplation of their ftructure and ufe, might feem to be more ftriking for their fingularity than their effect.

Utility of publifhing new inventions.

XIV. *Simple*

XIV.

Simple Proceſs for taking a Copy of a recent Manuſcript. Communicated to the Philomathic Society at Paris by CHARLES COQUEBERT.

Sugar is put in the ink. Damp unſized paper is laid on the writing. The impreſſion or copy is made by an heated iron.

THIS proceſs is the more intereſting as it requires neither machine nor preparation; and may be uſed in any ſituation. It conſiſts in putting a little ſugar in common writing ink, and with this the writing is made on common paper ſized as uſual, when a copy is required; this unſized paper is taken and lightly moiſtened with a ſponge. The wet paper is then applied to the writing, and a flat iron, ſuch as is uſed by laundreſſes, of a moderate heat, being lightly paſſed over the unſized paper, the counter-proof or copy is immediately produced *.

XV.

Narrative and Explanation of the Appearance of Phantoms and other Figures in the Exhibition of the Phantaſmagoria. With Remarks on the Philoſophical Uſe of common Occurrences.— W. N.

Phantaſmagoria. New application of the magic lanthorn by placing it on the oppoſite ſide of the ſcreen, and making the figures alone tranſparent.

A VERY ſtriking application of the magic lanthorn has been made this winter to the public amuſement by M. Philipſthal at the Lyceum. The novelty conſiſts in placing the lanthorn on the oppoſite ſide of the ſcreen which receives the images, inſtead of on the ſame ſide as the ſpectator, and ſuffering no light to appear but what paſſes through, and tends to form thoſe images. His ſliders are therefore perfectly opake, except that portion upon which the tranſparent figures are drawn, and the exhibition is thus conducted.

* I have not heard who was the original inventor of this uſeful proceſs. That ſugar prevents ink from ſpeedily drying has been long known, and this method of impreſſion has been uſed in ſome of our public offices, and elſewhere in England for ſome years.

W. N.

Account of the exhibition of Mr. Philipsthal.

All the lights of the ſmall theatre of exhibition were removed, except one hanging lamp, which could be drawn up ſo that its flame ſhould be perfectly enveloped in a cylindrical chimney, or opake ſhade. In this gloomy and wavering light the curtain was drawn up, and preſented to the ſpectator a cave or place exhibiting ſkeletons, and other figures of terror, in relief, and painted on the ſides or walls. After a ſhort interval the lamp was drawn up, and the audience were in total darkneſs, ſucceeded by thunder and lightning; which laſt appearance was formed by the magic lanthorn upon a thin cloth or ſcreen, let down after the diſappearance of the light, and conſequently unknown to moſt of the ſpectators. Theſe appearances were followed by figures of departed men, ghoſts, ſkeletons, tranſmutations, &c. produced on the ſcreen by the magic lanthorn on the other ſide, and moving their eyes, mouth, &c. by the well known contrivance of two or more ſliders. The transformations are effected by moving the adjuſting tube of the lanthorn out of focus, and changing the ſlider during the moment of the confuſed appearance.

Darkneſs. Thunder. Lightning.

Figures of the departed; phantoms, &c.

The figures ſeem to recede to an immenſe diſtance.

It muſt be again remarked, that theſe figures appear without any ſurrounding circle of illumination, and that the ſpectators, having no previous view or knowledge of the ſcreen, nor any viſible object of compariſon, are each left to imagine the diſtance according to their reſpective fancy. After a very ſhort time of exhibiting the firſt figure, it was ſeen to contract gradually in all its dimenſions, until it became extremely ſmall and then vaniſhed. This effect, as may eaſily be imagined, is produced by bringing the lanthorn nearer and nearer the ſcreen, taking care at the ſame time to preſerve the diſtinctneſs, and at laſt cloſing the aperture altogether: and the proceſs being (except as to brightneſs) exactly the ſame as happens when viſible objects become more remote, the mind is irreſiſtably led to conſider the figures as if they were receding to an immenſe diſtance.

How effected.

Transformations.

Several figures of celebrated men were thus exhibited with ſome transformations; ſuch as the head of Dr. Franklin being converted into a ſkull, and theſe were ſucceeded by phantoms, ſkeletons, and various terrific figures, which inſtead of ſeeming to recede and then vaniſh, were (by enlargement) made ſuddenly to advance; to the ſurprize and aſtoniſhment of the audience, and then diſappear by ſeeming to ſink into the ground.

Skeletons, &c. ſuddenly ruſh forward.

This

This part of the exhibition, which by the agitation of the spectators appeared to be much the most impressive, had less effect with me than the receding of the figures; doubtless because it was more easy for me to imagine the screen to be withdrawn than brought forward. But among the young people who were with me the judgments were various. Some thought they could have touched the figures, others had a different notion of their distance, and a few apprehended that they had not advanced beyond the first row of the audience.

The deception not equally effectual to all spectators.

As I have given this account, of an exhibition on which an ingenious mechanic in part depends for his support, it will not be impertinent to my present and future readers to add, that the whole, as well as certain mechanical inventions, were managed with dexterity and address, and that his gains in London have been very considerable. The figures for the most part are but poorly drawn, and the attempt to explain the rational object, or purpose of the exhibition was certainly well intended; but unfortunately for the audience his English was unintelligible. His lightning too, being produced by the camera was tame, and had not the brisk transient appearance of the lightning at the theatres, which is produced by rozin, or lycopodium powder, thrown through a light, which in Mr. P's utter darkness might easily have been concealed in a kind of dark lanthorn.

Observations the performance.

Lightning.

My young pupils on their return made drawings, and applied the magic lanthorn to a sheet in a door way between two rooms. Some of their drawings were made on thin paper and varnished, to render them transparent, and others were on glass. The paper figures were less bright than the others; but an advantage may be had in this material by those who cannot draw, because they may colour and varnish small figures, engraved in aqua-tinta or in any other manner without stroke.

Imitation by some young persons.

A plate of thin sheet iron, such as German stoves are made of, is an excellent instrument for producing the noise of thunder. It may be three or four feet long, and the usual width. When this plate is held between the finger and thumb by one corner, and suffered to hang at liberty, if the hand be then moved or shaken horizontally, so as to agitate the corner at right angles to the surface, a great variety of sounds will be produced; from the low rumbling swell of distant thunder, to the succession of loud explosive bursts of thunder from elevated clouds,

Remarkable imitation of thunder by a sheet of iron

clouds. This ſimple inſtrument is very manageable, ſo that the operator ſoon feels his power of producing whatever character of ſound he may deſire; and notwithſtanding this deſcription may ſeem extravagant, whoever tries it for the firſt time will be ſurprized at the reſemblance. If the plate be too ſmall, the ſound will be ſhort, acute, and metallic.

To Correſpondents.

The preſent ſeems to be no unfit place to notice ſome correſpondents which my late paper on ſhaving has induced to favour me with a few letters. To thoſe who find themſelves inſtructed and gratified by a ſmall addition to their daily comforts, I can only give my congratulation; but to that friend who calls himſelf *a Shaver*, but who I fear is no true ſhaver, from the little veneration he ſeems to have for the art, I am to return my thanks for his *Eſſay on the Art of cutting Bread and Butter*, and muſt ſay, that if it had abounded with inſtruction equal to its merriment, I ſhould have been glad to have given it to my readers. He who ſhall invent a machine to perform even this operation, will, I think, deſerve well of ſociety. I would humbly propoſe it as my opinion, not without expectation that many others may think with me, that no ſubject is beneath the conſideration of a philoſopher. Our beſt philoſophers have been moſt ſtudious of the daily occurrences of life. Newton's attention was attracted by the fall of an apple before he extended the theory of gravitation to the moon. Soap bubbles and the priſm were play-things before he ſelected them as inſtruments to analyſe the rays of light. Franklin by the kite of a child conducted lightning from the clouds to the earth: and in a word, it appears that the greateſt diſcoveries have been made, not by thoſe who could command the expenſive and ornamental apparatus of ſhowy experiment, but by ſuch as were in the habit of cloſe attention to the means, the agents, and the operations which are conſtantly performed around us, and frequently ill underſtood becauſe habitually neglected.

Ludicrous Eſſay.

Our beſt philoſophers have profited moſt from common occurrences.

Ridicule not injurious to the progreſs of philoſophy.

But laughter is ſaid to be one of the diſtinctive characters of the human ſpecies, and our pleaſures are not ſo many that we ſhould reject any innocent ſource of amuſement. No ſerious evil is to be apprehended from raillery, eſpecially if attended

tended with good nature. Herfchel will not be diverted from purfuing his difcoveries, becaufe the inhabitants of Laputa give themfelves too much anxiety about the fun's good health. Let the man of wit enjoy his joke, and the man of experiment his rational toys, provided both unite in cultivating that amiable fpirit of philanthropy, on which the happinefs of focial intercourfe fo much depends.

SCIENTIFIC NEWS.

Extract of a Letter from Dr. LORENZ DE CRELL. *(Helmftadt, Dec. 14, 1801.) On the Chlorophane and a fuppofed Variety of Barytes.*

THE Prince of Gallitzin, F.R.S. poffeffor of a moft excellent mineralogical cabinet, and living now as a private individual at Brunfwick, has obferved that the chlorophane, or that Siberian fluſs-fpar, which, after being gently heated on charcoal, turns green (on account of which it is called pyrofmaragdus in the Chemifche Annalen) and on being cooled regains its former colour; that the chlorophane, I fay, loft this property on being expofed in a tin box to the rays of the fun in a window during the fummer. It was turned quite colourlefs, and exhibited no light at all on being heated.— The Count of Hoffmanfegg, whofe travels to Portugal (for difcoveries in natural hiftory) were written by Profeffor Linck, his companion, has brought with him two ftones, greatly refembling quartz, or rock cryftal externally, but without regular figure; giving fparks with fteel, but of far greater fpecific gravity, viz. 3,500. It fhewed fome electricity on friction. This new kind of ftone (for fuch it is thought by good mineralogifts) is to be analyfed chemically. In the mean time Dr. Bruckman, who is well known by his books on precious ftones, thinks proper to call it *barytes nobilis.*

The chlorophane lofes its luminous property by expofure to light.

Barytes nobilis.

Corrindon or Adamantine Spar found near Philadelphia.

M. Guillemard, a learned mineralogift, who has travelled much in North America, writes to Dr. Delamétherie, that Mr. Adam Seybert has difcovered corrindon or adamantine fpar

Corrindon or adamantine fpar found in America.

ſpar at Cheſnut-hill, about ten miles from Philadelphia. It is known that the ſea coaſt in theſe parts is entirely granitic, and this granite extends to a conſiderable diſtance inland. Farther inland ſucceeds the gneiſs; then micaceous ſchiſtus; then grit ſtone; and, laſtly, the ground towards the lakes Ontario and Erie is all calcareous.

The granites of the coaſt of Philadelphia contain various marked veins of granite, as is obſerved in different granitic ſoils.

It was in one of theſe granitic veins, which may be almoſt compared to loads (filons), that Mr. Seybert found corrindon mixed with the other ſubſtances or elements of granite which compoſe the vein.

J. de Phyſ. Brum. 10.

Nature of the Earth which is eaten by the Inhabitants of New Caledonia, aſcertained by CIT. VAUQUELIN.

Savages on the Oronoko eat earth, which is thought to be nouriſhing.

Humboldt, in a letter to Fourcroy, of which an extract was given in the 50th number of the Bulletin des Sciences, affirms, that the Otomagues almoſt entirely ſubſiſt on a kind of earth for three months, when the Oronoko is ſo high that they can find no more turtle. Some of them eat a pound and a half daily. It is affirmed by the miſſionaries that they mix it with the fat of the crocodile's tail; but Mr. H. inſiſts that they do nothing but ſlightly burn and moiſten it. He thinks that moiſtened earth may be nouriſhing by decompoſing the air, or by ſome effect of the chemical affinities.

Inhabitants of New Caledonia alſo eat earth.

Cit. Labillardiere has ſtated a fact equally ſingular, from an obſervation made in a part of the world conſiderably diſtant from that inhabited by the Otamagues; that the inhabitants of New Caledonia, when preſſed by hunger, eat a conſiderably large quantity of a greeniſh ſteatites, which is ſoft and friable. It may be eaſily imagined how the dreadful cuſtom of eating priſoners of war might have been introduced amongſt a ſavage people, who are reduced to ſuch want as to be obliged to ſatisfy their hunger by diſtending their ſtomach and inteſtines with an earthy ſubſtance, which has no other alimentary quality but that of being light and friable.

Cit.

Cit. Vauquelin, wiſhing to know the nature of this earth, and to diſcover if it contained any nutritive parts, has analyſed it by the uſual methods, from ſome ſpecimens which were ſent him by Cit. Labillardiere. This, by analyſis, found to contain no nutriment.

This earth is ſoft to touch, compoſed of ſmall fibres, which are eaſily ſeparated, and when ignited it loſes $\frac{4}{100}$ of its weight. It is compoſed of,

37 parts of pure magneſia,
36 of ſilex.
17 of oxide.
3 or 4 of water.
2 or 3 of lime and copper.

It does not therefore contain any alimentary parts, and can only be conſidered as filling the ſtomach; a kind of mechanical expedient to ſuſpend the pains of hunger.

Soc. Philomath. No. 55, An. 10.

Account of a blue Oxide of Iron. By CIT. VAUQUELIN.

This ſubſtance was ſent to the Counſel of Mines, by the Baron de Molt. It is of a light blue colour, and has the form of ſmall inſulated maſſes in the cavities or clefts of quartz and hard green ſteatites. It is friable, but rather greaſy to touch, and changes its colour before the blow-pipe, and at laſt melts into a greeniſh white glaſs. Blue oxide of iron not yet analyſed.

Neither acids or weak alkalis alter its colour. Theſe circumſtances diſtinguiſh it from lapis lazuli and the pruſſiate of iron.

This ſubſtance communicates a ſaffron yellow to the muriatic acid in which it was digeſted, and changes its own colour a little, but cannot be diſcoloured unleſs it be at the ſame time diſſolved. There only remains a ſmall quantity of ſilex, which appears to have conſtituted its gangue.

On examining the muriatic acid that was uſed for this operation, it was found it had taken up alumine, lime, and oxide of iron, but neither ſulphurated hidrogen, manganeſe, nor phoſphoric acid were diſcovered in it, ſubſtances to which the blue colour of this oxide of iron might be aſcribed. It therefore remains to be determined what is the cauſe of the remarkable colour of this oxide, a colour which has never yet been

been given to this metal by any chemical procefs. It appears only that the iron in this oxide is carried to a degree of oxigenation nearly approaching to the *maximum*.

Bulletin de Sc. No. 55, *An.* 10.

Ufe of Whifkers in certain Quadrupeds. By M. VROLYK, *Profeffor of Natural Hiftory at Amfterdam.*

The whifkers of animals appear to be ufeful to feel their way in irregular cavities.

This naturalift has endeavoured to afcertain, by experiment, of what ufe the long ftiff hairs, termed *whifkers*, which are placed near the mouth of certain quadrupeds, might be to them. Having difpofed a number of books, placed on their edges, upon the ground in his chamber, fo as to form a kind of labyrinth, he let loofe a rabbit, with his eyes bound up, amongft them. The animal fucceeded in extricating himfelf from this labyrinth, without having overturned them; but when Mr. Vrolyk had cut off his whifkers, the animal, having no longer thefe tentacula to direct him, ran againft the books, and overturned them. Moreover it is known that the bulb, in which each of the whifkers is implanted, receives a fmall nervons filamen proceeding from the infra-orbilaty nerve.

Soc. Philom. No. 50, X.

Galvanic Charge of a large Battery by the Metallic Pile. By Dr. VAN MARUM.

The Harlem battery charged by galvanifm.

Dr. Van Marum, of Harlem, has charged 25 large electric jars (containing 137½ fquare feet of coating) by an almoft inftantaneous contact of a galvanic pile of 200 pair of 3 Guilder filver pieces, alternated with zinc and moift cloth. The electric intenfity was equal to that of the pile itfelf, but the fhock much lefs. The fhock of the battery was generally equal to that of half the number of pairs employed in its charge. It was, however, very ftrong, and reached up to the fhoulders. As the 120th Number of the Annales de Chemie, in which his letter is inferted, arrived late in the month, I muft defer the reft of the account.

ACCOUNT

ACCOUNT OF BOOKS OF SCIENCE.

A Syllabus of a Courſe of Lectures on Chemiſtry, delivered at the Royal Inſtitution of Great Britain. By Mr. Davy. 90 pages, 8vo. Cadell, London.—On Thurſdays and Saturdays at two, and on Thurſdays at eight o'clock, P.M. The morning lectures are read on general chemiſtry, and the evening lectures on the connection of chemiſtry with the arts.

The preſent courſe is divided into three parts. Part I. The chemiſtry of ponderable ſubſtances; under ſix diviſions. 1. Of the chemical powers, and the modes of their application. 2. Of undecompounded ſubſtances, or ſimple principles. 3. Of bodies compoſed of two ſimple ſubſtances. 4. Of bodies compoſed of more than two ſimple ſubſtances. 5. Of ſubſtances compoſed of different compound bodies, or of compound bodies and ſimple bodies. 6. Of the general phenomena of chemical action. Part II. The chemiſtry of imponderable ſubſtances; under four diviſions. 1. Of heat or caloric. 2. Of light. 3. Of the electrical influence. 4. Of galvaniſm. Part III. The chemiſtry of the arts; under eight diviſions. 1. Of agriculture. 2. Of tanning. 3. Of bleaching. 4. Of dyeing. 5. Of metallurgy. 6. Of the manufactory of glaſs and porcelain. 7. Of the preparation of food and drink. 8. Of the management of heat and light artificially produced. Davy's chemical lectures.

It is unneceſſary to make any general remarks on the benefits derived to ſcience from correct and ſufficiently extenſive outlines of facts and doctrines publiſhed from time to time by their active cultivators. The preſent work muſt prove eminently uſeful to the pupils of the lecturer, and will add to his reputation by its perſpicuity and order. In the rapid progreſs of experimental diſcovery, even thoſe learned men who attend to the general ſtate of natural philoſophy, without cloſely following its minuter variations, will be eſſentially ſerved by clear notices of the preſent ſtate of one of its chief departments.

The

The Elements of Book-keeping: comprifing a Syftem of Merchants' Accounts, founded on real Bufinefs, and arranged according to Modern Practice. By P. Kelly, *Mafter of Finfbury Square Academy.* Price 5s. Johnfon, Robinfons and Rivingtons.

Kelly's Elements of Book-keeping.

The improvements which time and experience have effected of book-keeping, do not comprehend any change in the original principle of double entry; but in the arrangement and claffification of fimilar accounts, which facilitate the operations of commerce nearly in the fame manner as the bufinefs of manufactures is expedited by the divifion of labour. The elementary treatife before us exhibits thefe improvements.

The author introduces his work by a concife account of the hiftory of this art, which he thus concludes: "from the foregoing view of the principal authors who have written on book-keeping, it appears that they have been compofed of two different defcriptions, poffeffing very diftinct qualifications. The firft, and by far the moft numerous clafs, were teachers who have explained the principles, without adverting to the progreffive improvements of practice; and the fecond, merchants who have exhibited thofe improvements, without explaining the principles. The productions of both claffes of writers are highly ufeful, and to combine their utility is the object of the prefent undertaking."

This work comprifes three fets of books, the firft explains and illuftrates the principles of both fingle and double entry; the fecond is a further exemplification of the Italian method of double entry by wafte book, journal, and ledger, which does not differ from the fyftems given by other authors; but the third fet of books differ effentially, and therefore deferves particular notice. Here the wafte-book is divided into a number of fubfidiary books, each of which is the regifter of its peculiar portion or department of bufinefs, and each book is divided into monthly tranfactions. By this means the journal is greatly fhortened and fimplified; but t principal advantage of this fyftematic arrangement confifts in pofting to the ledger; for there a whole month's cafh, bills, commiffion, infurance, or intereft, are each carried in one fum or entry from the journal to the ledger. This method, by which repetitions are avoided, and labour confiderably diminifhed, is now generally adopted in many principal mercantile houfes. The author informs

informs us in his preface, that " he has had accefs, for many years, to the books of feveral eminent merchants; and that he has endeavoured in all cafes to follow the moft approved precedents of mercantile experience."

In the third fet of books the invoices, fales, and other tranfactions, have been felected from merchants books:—the advantage of founding a fyftem of book-keeping on real occurrences are obvious; for although the principle of double entry can be explained by fictitious examples, yet thefe may give the learner wrong notions of bufinefs; whereas from real tranfactions, he will obtain fo much practical information of commercial affairs in general, as muft intereft the mind, and more effectually fix the attention in the courfe of ftudy.

Memoirs fur l'Influence de l'Air, &c.—Memoirs concerning the Influence of the Air, and feveral gafeous Subftances, on the Germination of various Kinds of Grain. By Huber and Senebier, 1 Vol. in 8vo. Geneva. Pafchoud, 1801.

Huber and Senebier on the influence of air on vegetation.

This work is curious from the circumftances under which it was compofed. Cit. Huber, already well known for his writings on Bees, is blind, notwithftanding which he has performed thofe experiments which were fuggefted to him by C. Senebier. The experiments were made in order to determine the influence of various gafes, particularly the oxigen gas on germination *. The feeds were placed either on wet flannels or fponges under receivers filled with gas; the principal refults were as follows. All the grains placed in the azotic gas remained unaltered, but germinated on being afterwards placed in the open air. Their growth was accelerated, though flightly, in pure oxigen, but was more vigorous in that which contained a little carbonic acid. In this experiment the carbon of the grain combines with the oxigen, and forms carbonic acid gas. The feeds fprung up in artificial atmofpheric air, the fame as they would have done in common air. The proportions moft favourable for germination, are three meafures of azote or hidrogen to one of oxigen. The grains which did not grow when included

* On this fubject fee alfo Cruickfhank, in the Phil. Journal, 4to, Vol. I. page 339, for Nov. 1797.

Huber and Senebier on the influence of air in vegetation.

in azote, were not affected by a proper quantity of oxigen being introduced by degrees, but ſhot forth very well when the ſame quantity was introduced at once. This difference may be explained by conſidering, that in the firſt caſe the oxigen is ſucceſſively employed in receiving from the grain the carbon which is diſengaged, ſo that none remains to vivify it, but when it is poured in at once, there is ſufficient for both purpoſes.

Seeds do not germinate either in carbonic gas, or pure hidrogen gas. One lettuce ſeed abſorbs in order to germinate a portion of oxigen, which is equal to 26 milligrammes of water, (half a grain) and it does not grow unleſs the oxigen is at leaſt the eighth part of the atmoſphere in which it vegetates. A great quantity of carbonic acid gas is more injurious to germination than the azote, and the azotic gas more ſo than hidrogen.

If ſeeds be put to grow in hidrogen gas, the carbon of the grains diſſolves in it, and combines very intimately.

The vapour of ſulphuric ether placed under a recipient of atmoſpheric air, prevented the ſeeds from growing, without altering the quantity of oxigen in the air. The ſame happens with camphor, oil of turpentine, aſſa-fœtida, vinegar and ammonia. Putrefied bodies prevent germination, by the abundance of car-bonic acid gas they emit. It appears from the preceding facts that oxigen is indiſpenſibly neceſſary to germination, and that it ſerves to carry off from the grain that carbon which is diſengaged by fermentation. But this rule is not without its exceptions.

In fact peas have germinated in water deprived of air by every poſſible means, and at whatever depth they have been immerſed. The ſeeds of beans, lentilles, ſpinach, lettuce and corn, grow in the ſame manner beneath the water with greater or leſs facility. Theſe ſeeds germinate better in water charged with oxigen gas, than in water entirely deprived of it, but not at all in water charged with carbonic acid. The acids retard their germination. The air emitted by peas under pure water, is a mixture of carbonic acid and carbonated hidrogen.

Peas have germinated in pure hidrogen gas and in air, where other ſeeds have already germinated, and they have totally exhauſted the oxigen it contained. In this experiment the hidrogen gas becomes loaded with carbon. They likewiſe

wiſe germinated in azote gas. They do not germinate in oil; but if after having been ſwelled in water they are put into oil, they germinate very well.

Theſe facts afford new inductions in favour of the decompoſition of water in germination, and conſequently in vegetation.

Bull. des Sc. x. No. 55.

Mémoires d'Agriculture, &c.—p. 79.

Memoirs of the Agricultural Society of the department of the Seine.

At the head of this volume are the order of the prefect for printing it, the rules of the Society, and a liſt of its ordinary and correſponding members, with an account of the Society's labours: after which are the following memoirs.—Inſtructions for preſerving wheat from ſmut: by CC. Cadet-de-Vaux, Parmentier, Saint-Genis, and Yvart.—Report of the experiments of C. Houdart, junior, on preparing and economiſing ſeed.—On the means of draining various lands by ſimple and not expenſive proceſſes, by C. Chaſſiron. This memoir, which has been publiſhed ſeparately, is preceded by ſome obſervations on the general ſyſtem of inland navigation in France, and followed by a comparative table of the coal-mines worked in each department, and of thoſe which require only navigable canals and rivers to enable them to be worked. The former are fifty-one in number; the latter, ſixty. Their importance to thoſe of the French manufactures that require fuel, or mechanic arts, and agriculture, muſt be obvious, at a time when they are perhaps not ſufficiently aware of the want of wood, with which they are threatened, from the great devaſtation of foreſts in moſt parts of the republic; a ſubject which has been treated of by C. Laſterie in a ſeparate memoir in this volume.—On the preciſe ſignification of the terms *agriculture* and *rural economy*, by C. Cels.—Some reflections on the ſuppoſed number of ſheep in France, by C. Delong. The intention of this memoir is to excite inquiry concerning this important ſubject.—The advantages of nurſeries on eſtates of a certain extent, in facilitating ſuch annual plantations as may be ſuited to them, by C. Villele.—An intereſting account of the ſucceſsful amputation of the fore-leg of a cow, which had been fractured, by C. Chaumontel.—Experiments, inquiries, and obſervations on elms, by C. Boucher; to which C. Denorgelles has added his experiments for obtaining alcohol from the ſap of vegetables.—On the product of different ſorts of wheat that are cultivated, and the melioration of them; or the advantages that would reſult from inquiring what ſorts of

wheat

wheat yield the moſt bread; by C. Chancey.—On rural edifices, by C. Garnier-Deſchênes.—On the manner in which the mountains in the Cevennes are fertilized, by C. Chaptal. —On the means of rendering fit for uſe wells that have been abandoned in conſequence of the mephitization of the ſoil, by C. Cadet-de-Vaux.—Obſervations on the working of mines, by C. Creuſé-Latouche.—On the cultivation of the ſugar-cane in the Caribbee iſlands, and particularly of that of Otaheite by C. Moreau-St.-Mery.—On the cultivation of the ſugar-cane, by C. Coſſigny.—On the cinnamon-tree of French Guiana, by C. le Blond.—The volume concludes with accounts of the lives of Cretté de Pallnel, and T. Francis de Grace.

BOOKS OF SCIENCE,

Imported by Deboffe, of Gerrard-Street, Saho.

Books of ſcience imported.

HISTOIRE Naturelle des Oiſeaux de Paradis, par Viellot, No. 13, 1l. 15s,

Dictionnaire raiſonné & univerſel des Arts & Métiers, par Jaubert, 1801. 5 Vol. 8vo. 1l. 11s. 6d.

Manuel du Voyageur à Paris, An. X. 2s. 6d.

Voyage pittoreſque & phyſico-economique dans le Jurat, par Lequinio, An. ix. 2 Vol. 12s.

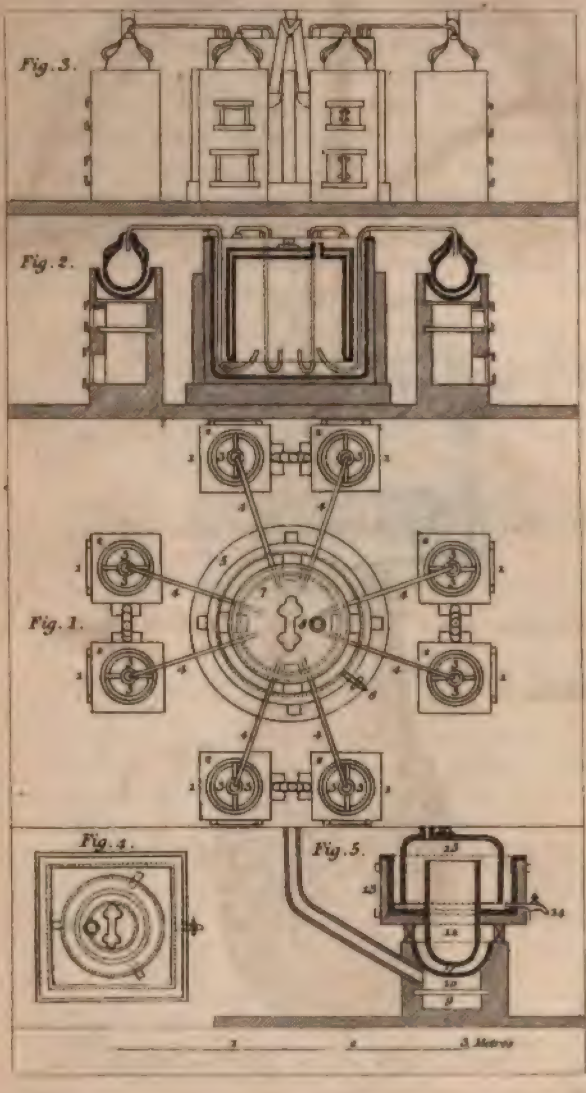
Geographie, moderne & univerſelle, par Lacroix, précédé d'une Traite de la Sphere & d'un d'Aſtronomie, 1800, 2 Vol. 8vo. avec Cartes, 18s.

Nouveau Dictionnaire François-Italien, & Italien-François, Abregé d'Alberti, 1801. 2 Vol. 8s.

Reſpecting agricultural information.

In anſwer to the obliging letter of *A friend to Agriculture*, recommending that ſubject to conſtitute part of the preſent Journal, I am to obſerve, that though this moſt intereſting department of human induſtry and reſearch is of the firſt importance to ſociety, and its proceſſes are ſcientific throughout, yet from the complicated number of cauſes to be developed, the conſiderable time required for experiment, the large ſcale of operation, the uncertainty of reporters, and other confidetions, I have been leſs forward to preſs this object, than from its high value I ſhould elſe have been. Communications, ſelections, or accounts, which from their authenticity and their deciſive effect, may tend to eſtabliſh points of eſſential value, will undoubtedly conduce to the purpoſes of our publication, and in this reſpect I ſhall thankfully avail myſelf of the liberal offer of my correſpondent.

Bleaching of Paper Stuff.



The Graphometer of Carangeau.

Fig. 1.

Dr. Thompson's Experiments

To shew that Fluids are not thrown into Currents by heat.

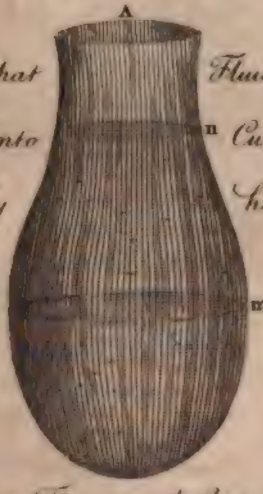


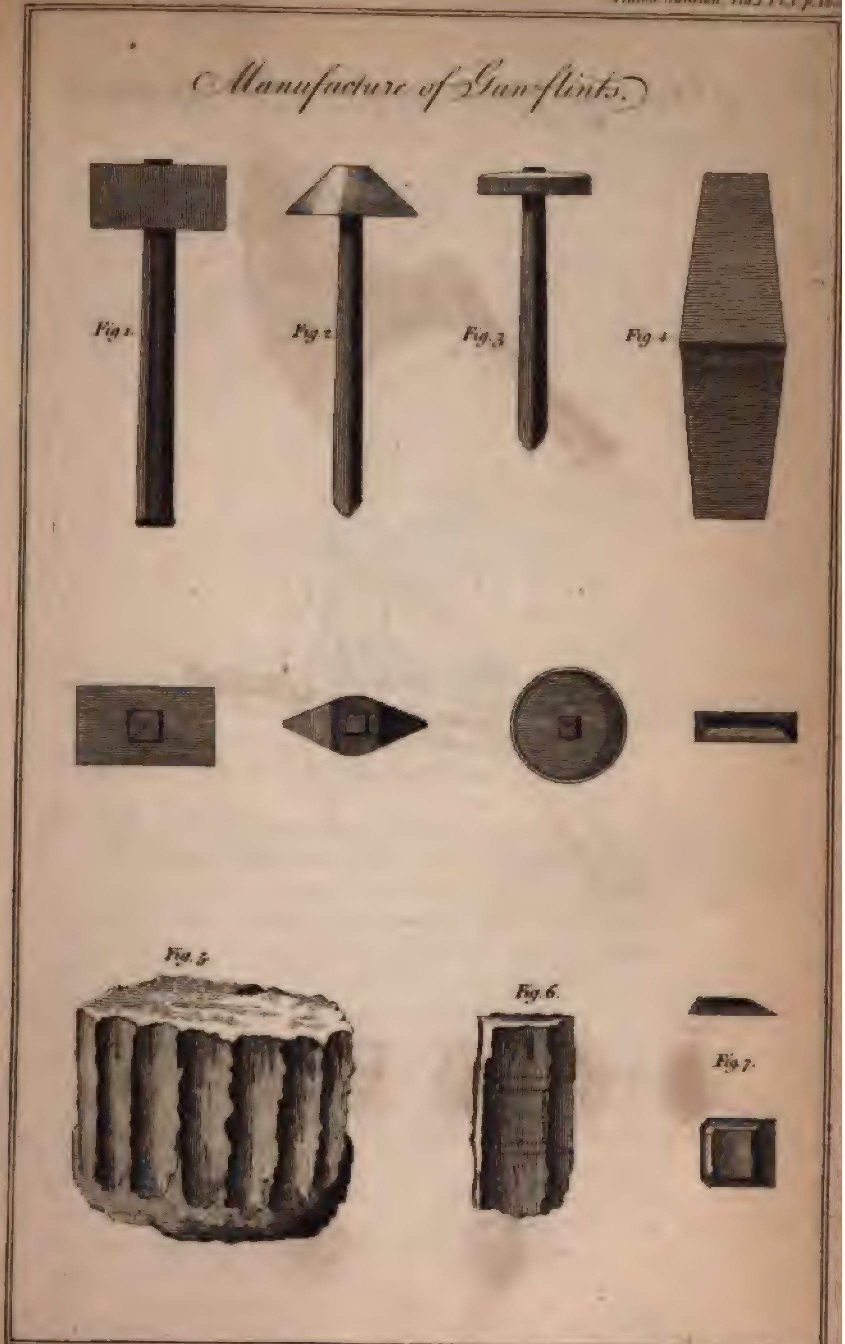
Fig. 2.

Volta's Theory of Galvanism.

Fig. 3.

Cure of Deafness by puncture of the Memb: tympani.

Mutlow sc Russell Ct.



Mutlow sc. Russell Ct.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1802.

ARTICLE I.

Description of an Engine which operates by the Pressure and Descent of a Column of Water against a Piston; nearly in the same Manner as the double Steam Engine operates by Means of Steam. Communicated by the Inventor Mr. RICHARD TREVITHACK, *of Camborne, near Truro, in Cornwall.*

Application of an inclosed column of water as a first mover.

IN a late conversation with the inventor of the engine which forms the subject of the present memoir, the method of applying a column of water as a first mover, by its alternate action upon a piston in a cylinder, was mentioned by me as likely to prove advantageous in a variety of situations, and probably to afford as beneficial a result with regard to power, as any of the other ways of applying a fall of that fluid. It was with no small gratification, that I found this skilful engineer had amply considered the subject, and had carried it into full effect. I immediately saw, that the communication of results so interesting and authentic, would be highly acceptable to my readers, and honourable to the Journal. Upon my request, he with great readiness and liberality supplied me with the drawing exhibited in Plate IX. and had the goodness to dictate the materials, whence the following description was made.

 The

History of a new engine for raising water by an inclosed column.

The engine, of which a section is here given on a scale of half an inch to a foot, was erected three years ago at the Druid Copper Mine, in the parish of Illogan, near Truro, in Cornwall.

Description of the parts, and their organization.

It was worked about two years; but the mine has not proved sufficiently beneficial to encourage the proprietors to go on during the last twelvemonth. A B represents a pipe six inches in diameter, through which water descends from the head to the place of its delivery to run off by an adit at S, through a fall of 34 fathom in the whole; that is to say, in a close pipe down the slope of a hill 200 fathoms long, with 26 fathoms fall; then perpendicularly six fathoms, till it arrives at B, and thence through the engine from B to S two fathoms. At the turn B the water enters into a chamber C, the lower part of which terminates in two brass cylinders four inches in diameter; in which two plugs, or pistons of lead D and E, are capable of moving up and down by their piston rods, which pass through a close packing above, and are attached to the extremities of a chain leading over, and properly attached to the wheel Q, so that it cannot slip.

The leaden pieces D and E are cast in their places, and have no packing whatever. They move very easily; and if at any time they should become loose, they may be spread out by a few blows with a proper instrument, without taking them out of their place. On the sides of the two brass cylinders, in which D and E move, there are square holes communicating towards F and G, which is an horizontal trunk or square pipe, four inches wide and three inches deep. All the other pipes G, G and R are six inches in diameter, except the principal cylinder wherein the piston H moves; and this cylinder is ten inches in diameter, and admits a nine foot stroke, though to accommodate the drawing to our Journal, it is here delineated as if the stroke were only three foot.

The piston rod works through a stuffing box above, and is attached to M N, which is the pit-rod, or a perpendicular piece divided into two, so as to allow its alternate motion up and down and leave a space between, without touching the fixed apparatus or great cylinder. The pit-rod is prolonged down into the mine, where it is employed to work the pumps, or if the engine were applied to mill work, or any other use, this rod would form the communication of the first mover.

K L,

K L, is a tumbler or tumbling bob, capable of being moved on the gudgeons V, from its prefent pofition to another, in which the weight L fhall hang over with the fame inclination on the oppofite fide of the perpendicular, and confequently the end K will then be as much elevated as it is now depreffed. New engine for raifing water by the defcent of a column inclofed in a pipe.

The pipe R S has its lower end immerfed in a ciftern, by which means it delivers its water without the poffibility of the external air introducing itfelf; fo that it conftitutes a torricellian column, or water barometer, and renders the whole column from A to S effectual, as we fhall fee in our view of the operation.

The operation. Let us fuppofe the lower bar K X of the tumbler to be horizontal, and the rod F O fo fituated, as that the plugs or leaden piftons D and E fhall lie oppofite to each other, and ftop the water ways G and F. In this ftate of the engine, though each of thefe piftons is preffed by a force equivalent to more than a thoufand pounds, they will remain motionlefs, becaufe thefe actions being contrary to each other, they are conftantly in equilibrio. The great pifton H being here fhewn as at the bottom of its cylinder, the tumbler is to be thrown by hand into the pofition here delineated. Its action upon O F, and confequently upon the wheel Q, draws up the plug D, and depreffes E, fo that the water way G becomes open from A B, and that of F to the pipe R: the water confequently defcends from A to C; thence to G G G, until it acts beneath the pifton H. This preffure raifes the pifton, and if there be any water above the pifton, it caufes it to rife and pafs through F into R. During the rife of the pifton, (which carries the pit-rod M N along with it) a fliding block of wood I, fixed to this rod, is brought into contact with the tail K of the tumbler, and raifes it to the horizontal pofition, beyond which it overfets by the acquired motion of the weight L. Operation, or working procefs.

The mere rife of the pifton, if there were no additional motion in the tumbler, would only bring the two plugs D and E to the pofition of reft, namely to clofe G and F, and then the engine would ftop; but the fall of the tumbler carries the plug D downwards quite clear of the hole F, and the other plug E, upwards quite clear of the hole G. Thefe motions require no confumption of power, becaufe the plugs are in equilibrio, as was juft obferved.

New engine for raising water by the descent of a column included in a pipe.

In this new situation the column A B no longer communicates with G, but acts through F upon the upper part of the piston H, and depresses it; while the contents of the great cylinder beneath that piston are driven out through G G G, and pass through the opening at E into R. It may be observed, that the column which acts against the piston is assisted by the pressure of the atmosphere, rendered active by the column of water hanging in R, to which that assisting pressure is equivalent, as has already been noticed.

When the piston has descended through a certain length, the slide or block, at T, upon the pit-rod, applies against the tail K of the tumbler, which it depresses, and again oversets; producing once more the position of the plugs D E, here delineated, and the consequent ascent of the great piston H as before described. The ascent produces its former effect on the tumbler and plugs; and in this manner it is evident that the alternations will go on without limit: or until the manager shall think fit to place the tumbler and plugs D E in the positions of rest; namely so as to stop the passages F and G.

The length of the stroke may be varied by altering the positions of the pieces T and I, which will shorten the stroke the nearer they are together; as in that case they will sooner alternate upon the tail K.

As the sudden stoppage of the descent of the column A B, at the instant when the two plugs were both in the water way, might jar and shake the apparatus, those plugs are made half an inch shorter than the depth of the side holes; so that in that case the water can escape directly through both the small cylinders to R. This gives a momentary time for the generation of the contrary motion in the piston, and the water in G G G, and greatly deadens the concussion which might else be produced.

Former attempts to make pressure engines, upon the principle of the steam engine, have failed, because water not being elastic could not be made to carry the piston onwards a little, so as completely to shut one set of valves and open another. In the present judicious construction the tumbler performs the office of the expansive force of steam at the end of the stroke.

Rate of work by these engines.

There are several other engines of this construction at work. The general rate of working of all these engines is, that four feet

The Graphometer of Carangeau.

Fig. 1.

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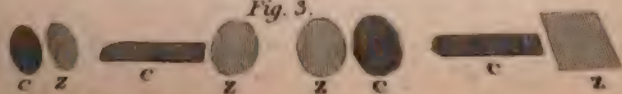
To shew that Fluids are not thrown into Currents by heat.

Fig. 2.

Volta's Theory of Galvanism.

Fig. 3.



sion, that they conduct caloric in a very imperfect degree. From a series of experiments which I have undertaken on this subject, I have been convinced, that investigations respecting it are liable to sources of error more important, and more difficult to be avoided than might *a priori* be supposed, and that we have no experiments sufficiently unobjectionable to decide the question. The observations I shall have to offer in the following memoir, will I trust justify these assertions.

but the experiments offered to decide the question abound with difficulties.

Communication of heat through solids;

When caloric is communicated to the surface of any solid substance, the temperature of the whole mass is increased with more or less celerity, and becomes uniform. In this case the caloric passes from one particle of the solid to another, and is thus equally distributed through the whole. When caloric is communicated to a mass of fluid, its temperature is likewise uniformly increased, and in this case also it was supposed, that the caloric is transmitted through the fluid, as in the former example through the solid. Count Rumford denies the possibility of such a transmission, and conceives the change of temperature in the fluid to be produced in another mode.

and fluids.

Circulation of fluids from heat.

When any portion of a fluid is heated it must necessarily be expanded, its specific gravity must be diminished, and from its mobility it must change its place. If therefore caloric be communicated through a solid surface to a fluid, the portion of fluid immediately in contact with the solid, when heated, will acquire a specific gravity different from that of the mass; if the caloric is communicated from beneath, the heated portion of fluid must ascend, its place will be occupied by another portion, and thus the whole mass will be successively brought into contact with the heated surface, and will have its temperature increased. It is in this mode only according to Count Rumford, that the temperature of fluids can be changed. They can receive caloric from any solid matter with which they are in contact, but the heated portion communicates no part of its caloric to the rest of the mass, and the change of temperature which the whole suffers is owing solely to the motions of its parts.

The circulation does not *absolutely* prove the Count's opinion.

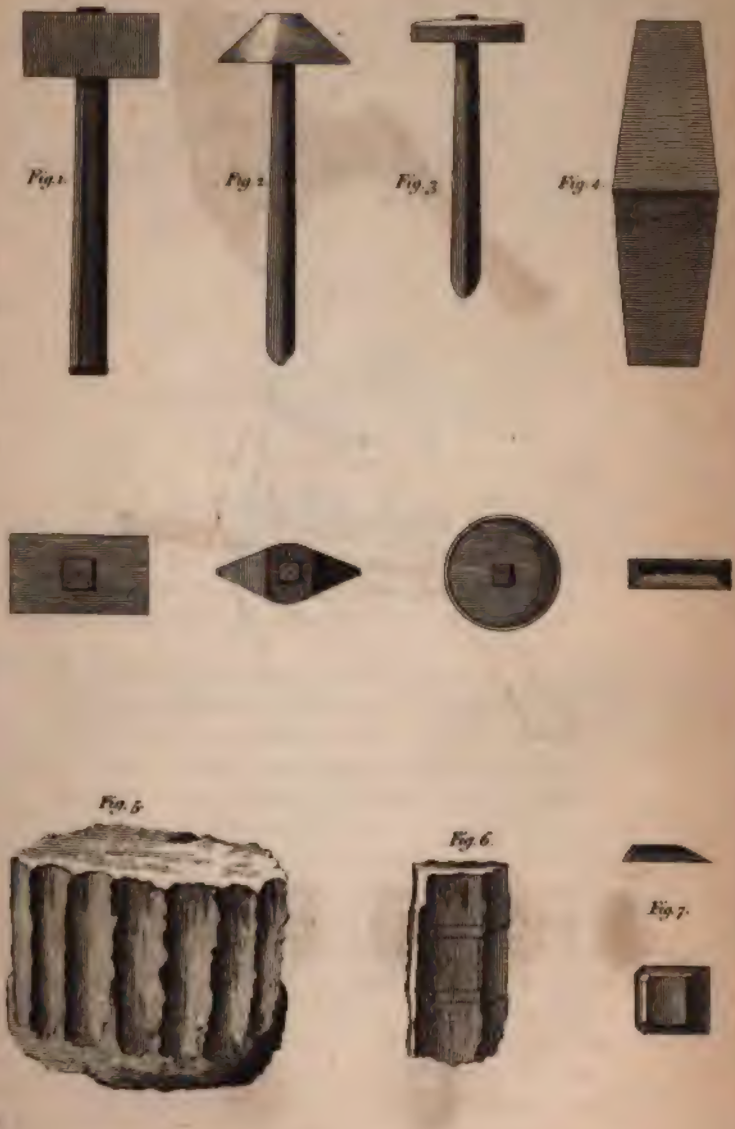
That it is in a great measure owing to this cause, has been clearly demonstrated by Count Rumford's experiments. He has shewn, that when heat is applied to a fluid, motions of its parts take place, and that whatever retards or obstructs these motions, diminishes the celerity with which the temperature

is

Cure of Deafness by puncture of the Memb: tympani.

Mutlow sc Russell Co.

Manufacture of Gun-flints.



Muxlow sc Russell Ct

It cannot be known whether this be all that happens.

It may be supposed perhaps, that the quantity of caloric thus conveyed by the vessel would not be sufficient to produce the rise of temperature which took place in these experiments. But how is this to be ascertained. It may appear probable, but until it is proved, the point must remain doubtful, and the experiments must be therefore inconclusive.

To surround the vessel externally with water

It was an obvious idea, that this source of error might perhaps be obviated in a great measure by surrounding the vessel with water, which would carry off the caloric conveyed by its sides. It is evident, however, that it could carry off only part of it, for part must still continue to be abstracted from the internal surface by the water in contact with it. And even the quantity taken from the external surface of the jar by the water surrounding it, would be only somewhat greater than what would be carried off by the asmosphere when the experiment was performed as described above.

promised to be of some use. Experiment. Heat was found to descend in the internal and external water.

The experiment however of surrounding the vessel with water might afford some information: it would at least prove, that caloric was conveyed by the sides of the vessel, and it might indicate in some measure the quantity conveyed. I accordingly performed it in the manner represented in Fig. 2. D being merely a cylindrical glass vessel six inches in diameter, in which the apparatus used in the former experiments was placed. Water was poured into it, to the same height as in the internal vessel, and a thermometer was suspended in the water, the bulb being at precisely the same distance from the surface, as the bulb of the bent thermometer in the vessel A. The brass ball heated to 212° was suspended in the water in the latter vessel, and at the same distance from the thermometer as in the preceding experiments. The following were the results. At the commencement of the experiment, both thermometers were at 46°.

In 3 minutes, the internal thermometer A $46\frac{1}{4}$ the external E still 46

5 min.	$47\frac{1}{4}$	$46\frac{1}{2}$
10	$48\frac{1}{2}$	$47\frac{1}{4}$
15	$49\frac{1}{2}$	$48\frac{1}{4}$
20	$49\frac{1}{2}$	$48\frac{1}{4}$
30	49	48

Many reasons why this experi-

This experiment proves nothing more, than that a quantity of caloric is conveyed by the sides of the vessel It might perhaps

perhaps be ſuppoſed, that by comparing the riſe of temperature in the two thermometers, a concluſion might be drawn with reſpect to the mode in which the caloric was conveyed to them. If it were intirely conveyed by the ſides of the veſſel, ſince as much ought to be given out by the external as by the internal ſurface, the water without ſhould be heated as much as that within, and conſequently the two thermometers ſhould riſe equally, and to the ſame extent. But if the thermometer in the inner veſſel roſe more than that in the outer, it might be ſuppoſed, that the interpoſed fluid had directly conveyed to it part of the caloric from the heated ball above, with which it was more nearly in contact. But to eſtabliſh ſuch a concluſion from any differences of this kind, certain circumſtances are indiſpenſible, which are abſolutely unattainable. It is requiſite, for example, that both thermometers ſhould be preciſely on the ſame level, at the ſame diſtance from the ſides of the veſſel, and covered with the ſame quantity of water occupying the ſame volume. The ſmalleſt variation in theſe circumſtances would produce a variation in the reſult. But if they were even obtained alike, the ſituation of the veſſels muſt ſtill remain very different; the one for example, preſents a much more extenſive ſurface to the ſurrounding medium than the other, and the nature of that medium is different, the internal veſſel being ſurrounded with water, the temperature of which is augmenting while the experiment continues, the external with atmoſpheric air, whoſe temperature remains the ſame. It is impoſſible to eſtimate the differences in effect which muſt ariſe from theſe differences in ſituation, and conſequently no concluſion can be drawn from the comparative alterations of temperature in the reſpective thermometers, as to the quantity of caloric conveyed to either of them by the ſides of the veſſel.

ment cannot be made deciſive.

The preceding experiments then, or any of a ſimilar nature, are incapable of determining the queſtion reſpecting the conducting power of fluids. In all of them a quantity of caloric is conveyed by the ſides of the veſſel in which the experiment is made, and this quantity we have it not in our power to aſcertain, ſo as to determine whether it is adequate or not to produce the augmentations of temperature which actually take place, and conſequently whether any part of that augmentation is to be aſcribed to a conducting power in the fluid.

The ſides of the veſſel will always conduct heat;

and no precaution can insure the object of the experiment.

Every precaution that can be taken to obviate this source of error, defeats the purpose of the experiment itself, since such precautions tend equally to lessen the effect which would result from a conducting power in the fluid supposing it to possess it, and it must therefore remain uncertain to what the diminished effect which takes place where they are employed, is to be attributed.

Other remarks.

If for example, the bulb of the thermometer be placed at a great distance from the matter communicating caloric, it will be a longer time before the thermometer begin to rise, and it will also not rise to so great a degree. But which ever opinion be adopted, whether that the fluid directly conducts caloric, or that caloric is communicated only by the vessel, this ought to be the case, since the circumstances necessary for its transmission in either way are rendered more unfavourable.

In like manner when a wider vessel is employed to contain the fluid, the rise of temperature must be less than when a narrow one is used, because in this case also there must be a larger portion of interposed fluid to be heated.

Or when a large portion of fluid is placed above the bulb of the thermometer, and in contact with the substance communicating the caloric, as with the ball in the preceding experiments, the same diminution of effect must take place, because the caloric given out by the ball being absorbed by a larger quantity of fluid, the temperature of that fluid must be less increased, and it must have less effect either in heating the sides of the vessel, or the thermometer.

General observation or result.

It appears therefore that in all experiments of this kind, a source of fallacy must be present: the effects of this will be more or less considerable according to the circumstances of the experiment, but they cannot be intirely obviated, nor their extent appreciated, so as to admit of a certain conclusion being drawn.

History of the experiments.

It may not be improper to remark, that the preceding experiments were made last winter, were stated in my course of lectures, and a concise account of them published in my Elements of Chemistry. I have now stated them partly to shew the insufficiency of experiments of this kind, either to establish or controvert Count Rumford's opinion, but principally as an introduction to those I have further to relate, and which appear to me to be free from the source of error I have stated.

This

This, of which I at one time despaired, may be attained by the simple contrivance of employing a vessel of ice. If a cylindrical vessel of ice be procured similar to that used in the preceding experiments, and if a thermometer be fixed in it, if on filling it with a fluid at the temperature of 32°, and suspending in it, at a short distance from the bulb of the thermometer, a heated solid, any rise of temperature take place, it may be considered as a certain proof of a conducting power in the fluid. No caloric could possibly be conveyed to it by means of the vessel, since ice cannot have its temperature above 32°, and there is no other mode by which the caloric can pass from the ball to the thermometer than by the interposed fluid. Of the experiments made to determine this point, an account will be given in a subsequent memoir.

A vessel of ice at 32°, containing water at 32° will not raise the temperature of the fluid during its own fusion.

III.

Letter from Dr. VAN MARUM *to Mr.* VOLTA, *Professor at Pavia, containing Experiments on the Electric Pile, made by him and Professor* PFAFF, *in the Teylerian Laboratory at Haarlem, in November,* 1801.*

THE weather and season of the year preventing these gentlemen from attempting to charge the whole of the great Haarlem battery, consisting of 100 jars, in general nearly a line thick, and containing five square feet and a half of coating each, with the galvanic pile, they took five-and-twenty of these jars, which they charged separately, a few at a time, and all together; and uniformly found the single jars or the batteries charged to the same degree of intensity as the pile. They had taken twenty-six jars, but one of them did not receive the charge well, which they ascribe to the too great thickness of the glass.

Van Marum's experiments on the galvanic pile.

They next charged the battery of 137½ square feet with a greater or less portion of the pile, by soldering a hook to every twentieth plate of zinc, to which the insulated metallic wire for producing the communication between the battery and the pile might be conveniently attached. Beginning at

Battery of 137½ square feet charged by it,

* Abridged from the Annales de Chimie, No. 120, Vol. XL. p. 289.—C.

the

taking its electricity at different heights.

the fortieth pair from the bottom, the firft where the feparation of Bennet's electrometer was diftinctly perceptible, the battery was charged to the fame intenfity as the electrometer indicated when brought into contact with that part of the pile, giving here a feparation of one line. The effect was precifely analogous when the battery was charged by fimilar inftantaneous contact with the pile at the fixtieth, eightieth, or hundredth pair, and fo on.

The zinc being at top, the electricity pofitive.

In this pile, the filver being the lowermoft of the metals, the electricity was pofitive at top, and the electricity communicated to the interior furface of the battery was the fame, the contact being made with the fuperior part of the pile.

The metals being inverted, the electricity charged.

The order of the metals was then inverted, and the experiments repeated at different heights of the pile as before, with fimilar refults.

Shocks taken by copper conductors two inches in diameter from the battery thus changed.

They now proceeded to examine the fhocks given by the battery charged at different heights of the pile; for which purpofe they employed two conductors of copper, two inches in diameter, held in wet hands. Beginning with the battery charged at the twentieth pair, they very diftinctly perceived the paffage of the ftream from the conductor into the hand, and from the hand into the conductor. One gentleman prefent, Mr. Vander Ende, felt it as far as the wrifts. When the battery was charged by forty pairs, real fhocks were felt at the wrifts: when by fixty, the fhocks were very perceptible at the elbows: and they gradually increafed, till the fhocks extended to the fhoulders with confiderable ftrength, when the battery was charged by the whole pile.

The fhocks given by the pile itfelf much ftronger.

Thefe fhocks, however, were not equal to thofe given by the pile itfelf, but only about half their ftrength; the fhock from a hundred pairs appearing to be equal to that of the battery when charged by two hundred.

Comparative experiments made with an electrical machine.

Having continued thefe experiments till nothing more could probably be learned from them, the comparative effects of the electrical machine, confifting of a plate of glafs thirty-one inches in diameter, were tried with the fame battery.

Precautions ufed in conducting thefe experiments.

This experiment requiring particular precautions, that the contact of the conductor might not impart more electricity to the battery than was furnifhed by the action of the machine during the contact, Dr. Van Marum formed a communication between the conductor and the ground by touching it with one

one of his fingers, and withdrew his finger the moment he brought the large wire into contact with the conductor. This manœuvre he practiſed repeatedly, till he had acquired a habit of doing it with preciſion, before he began his comparative experiments; ſo that he was well aſſured, that the contact of the conductor, by means of the wire, took no more of the fluid than was furniſhed by the machine at the inſtant of contact. A ſingle contact would not give the battery a change, that the electrometer was capable of indicating; but by repeated experiments it was found, that ſix of theſe contacts of the conductor imparted to the battery the ſame intenſity as one contact of the pile. The power of the machine being equal to about half what the great machine poſſeſſed in its former ſtate, from 1785 to 1789; but the great machine having gained conſiderably, particularly for charging batteries, by the rubbers of the new conſtruction applied in 1790, and by the uſe of Kienmayer's amalgama, ſo that its power was rendered the quintuple of what it was before; the power of the pile of 200 pairs is to that which the great Teylerian machine poſſeſſes at preſent as 3 to 5. The ratio could not be examined directly, becauſe a fire cannot be made in the muſeum, and the ground on which it ſtands is very damp in winter: this, therefore, is deferred till ſpring.

The pile of 200 pairs had ſix times the power of the machine with a plate 31 inches in diameter, and three-fifths of that the great Haarlem machine.

The ſhocks given by the battery, charged to different degrees of intenſity by contacts with the conductor of the electric machine, were now compared with thoſe it gave with ſimilar intenſities obtained from the pile; and reiterated trials convinced the experimentaliſts, that there was no perceptible difference between the ſenſations or ſhocks in the two inſtances, provided the intenſities were the ſame. Hence Dr. Van Marum concludes, as the effects of ſuch a conſiderable battery, when charged by the galvanic pile, are preciſely ſimilar, in every reſpect, to its effects when charged by a powerful electrical machine, the identity of the fluid put in motion by the pile with that moved by the electrical machine is proved ſo deciſively, that no one will queſtion them in future. Theſe experiments, he adds, joined with thoſe of Profeſſor Volta, render extremely diſputable, or rather completely refute the exiſtence of a peculiar fluid, in all the other experiments termed galvanic.

The ſhocks given by the battery, charged by the machine, not at all different from thoſe it gave when charged by the pile to the ſame height: the two fluids therefore *preſumed* to be the ſame.

Theſe

Extreme velocity of the fluid.

Thefe experiments have farther proved in a decifive manner, and on a large fcale, another important circumftance refpecting the pile; namely, that the ftream of fluid moved by it has a velocity furpaffing all conception; fince a battery of 137½ fquare feet was charged to the intenfity it difplayed by a contact as fhort as poffible with the wire of communication, a contact that did not continue one-twentieth of a fecond. After this it is no way aftonifhing, that the pile, by a continuance of its action, fhould produce fuch effects as have never been feen where common electrical machines are employed, as the fpeedy decompofition of water for inftance. Certainly no other electrical machine known, the grand Teylerian machine excepted, can furnifh a continued ftream at all approaching that of the pile; on which account it is a powerful mean of producing feveral effects, which may contribute greatly to the progrefs of natural philofophy.

Hence the effects of the pile fuperior to thofe of common electrical machines;

and may be of great ufe in phyfics.

Method of infulating the pile.

This confideration induced Dr. Van Marum to endeavour to augment the power of the pile. From the beginning he was careful to infulate it more effectually than is commonly done. For this purpofe he placed it on a thick cake of lac, and kept it in its vertical pofition by fticks of fealing-wax two inches long, fixed horizontally in four flips of wood, furrounding the pile. Thefe fticks of fealing-wax were fixed to little wooden cylinders, paffing through holes in the uprights at every four inches, and kept in their places only by being made to fit with fuch a degree of tightnefs, as would allow them to be moved backward or forward, fo as to fuit piles of different diameters. The uprights were fixed at top and at bottom in a piece of wood a foot fquare. Profeffor Pfaff himfelf was aftonifhed at the great effect of the firft pile compofed of three Guilder pieces, plates of zinc of the fame diameter, being an inch and a half, and bits of cloth moiftened with a faturated folution of muriate of ammoniac. In one of the experiments the end of an iron wire, No. 16, $\frac{2}{240}$ of an inch in diameter, was made red-hot for the length of a line, and even fufed at the extremity, by a pile of fixty pairs.

Wire fufed by it.

Having read an account of the experiments of Fourcroy, Vauquelin, &c. on the fufion of wire by large plates of copper and zinc, Dr. Van Marum procured thirty-two of each metal, five inches fquare, and made a pile with them; firft of

of ten inches in diameter, and eight pairs in height, by placing four of each metal together as one plate, they being exactly square, and their furfaces very fmooth, that they might be in clofe contact; and then of five inches in diameter, and thirty-two pairs in height. The power of the higher pile greatly exceeded that of the larger. In Auguft, Dr. Van Marum had fufed completely into globules five inches of the iron wire mentioned above, and made feven inches red-hot, with this pile.

Pile of ten inches in diameter.

Another of five.

The powers of piles depend more on their height, than on their diameter.

The number of pairs of metal being increafed, the effect was not; fifty pairs producing lefs than twenty-five, by which nine inches of the fame wire were melted when Profeffor Pfaff was prefent in November. This they afcribe to the moifture being too much preffed out of the pieces of pafteboard by the weight of fo many plates of metal a line and a half or two lines thick; as the upper twenty-five pairs, being taken off, had as much effect as before, while the lower twenty-five had not half the power. In confequence they divided the pile into four, containing together 110 pairs. The plate of copper, placed under and connecting the two piles that were infulated, had a rim, that the folution preffed out of the pafteboard might remain in it, and not wet the infulating cake, and deftroy the infulation. Thefe two piles, containing fixty pairs fomewhat thinner than the others, when not connected with the other two, made fix inches of the wire red-hot. The other two piles, containing fifty pairs, made eight inches very red, and fufed great part of this length. This difference was afcribed to the pafteboards not being fufficiently moiftened in the former inftance. The four piles connected together made twelve inches of the wire red-hot.

Effects not increafed in proportion to the height, when the plates are large and thick, becaufe too much fluid is preffed out.

On this account the pile fhould be divided into feveral, communicating with each other.

On taking fparks repeatedly at each extremity of the connected piles, by means of a wire communicating with a bafin containing quickfilver, no difference between thofe of the pofitive and thofe of the negative electricity could be difcerned. Whether iffuing, or entering, they appeared radiating, when iron wire was employed; which the experimentalifts afcribe to the combuftion of the iron, for when a wire of platina was ufed, no rays were perceptible in the fparks of either electricity. Sometimes the communication was made by means of a needle, faftened to the flender wire

Pofitive and negative fparks did not differ.

 which

which was fixed to the end of the conducting wire; at other times by the lefs pointed end of the conducting wire itfelf.

Beautiful phenomena produced by the combuftion of of the wire.

When the furface of the quickfilver was touched with the end of a flender wire inftead of the needle, a very ftriking phenomenon enfued: the combuftion of the extremity of this wire was effected with fuch force, that it threw out fparks on all fides, which formed thoufands of apparent rays, reprefenting beautiful funs fome inches in diameter. By gently lowering the end of the wire in proportion as it was difperfed by the combuftion, this appearance could be continued at will. It is feen in fome degree likewife when the quickfilver is touched with the point of a fine needle; but then it is of fhort duration, as it ceafes as foon as the needle has left its point. The experiment was repeated with wires of $\frac{1}{240}$, $\frac{1}{151}$, and $\frac{1}{185}$ of an inch in diameter: that of the middle fize gave the largeft and moft brilliant funs, but the fmaller fucceeds better when the power of the pile is lefs.

Quickfilver oxided on its furface by the fparks.

On taking wires too thick to be melted, the oxidation of the quickfilver at its furface by each fpark was more diftinctly feen, fpots of upward of a line in diameter being formed.

Platina fufed by the pile.

The extremity of a wire of platina, about $\frac{1}{175}$ of an inch in diameter, was melted, and formed a globule.

Sparks upward of a line in diameter.

The fparks iffuing from the end of the communicating wire, when it was not too flender, were more than one-tenth of an inch in diameter.

Piles of an equal number of pairs, though different diameters, give equal intenfities, and fimilar fhocks; notwithftanding they differ fo much in fufing metals.

On comparing the intenfity of the electricity of this pile with that of the former of zinc and filver an inch and a half in diameter, both by the fimple contact of the Doctor's moft fenfible electrometer, and by the condenfer and an electrometer of lefs fenfibility, they were found precifely the fame, when the fame number of pairs were ufed: they charged the battery before defcribed to the fame height: the fhocks given by the battery thus charged were in both cafes precifely the fame: and in thofe from the piles the difference was fcarcely perceptible.

Attempt to account for this.

It appears ftrange, that two piles fo equal as to intenfity, and charging large batteries in fuch an equal degree, fhould produce fuch different effects in fufing metals. To form a juft notion of this, obferves Dr. Van Marum, we muft diftinguifh the action of a pile which is infulated from that of one which is not. In the latter there is a ftream paffing continually

from

from one extremity to the other, and through the conductor or chain of conductors which connects the two extremities: but this ſtream cannot take place in an inſulated pile. From this ſingle diſtinction it appears, that the equality of the intenſities of inſulated piles affords no reaſon to expect an equality of the ſtreams of the two piles when not inſulated: on the contrary, if the ſtream meet with leſs impediment in a large pile, than in one of ſmaller diameter, the ſtream will produce a greater effect, in conſequence of its greater velocity.

To ſee how far this velocity depended on the ſize of the pieces of paſteboard employed, two piles were conſtructed, each of twelve pairs of the large plates, in one of which the paſteboard was cut to the ſize of the plates, in the other to the diameter of half an inch only. The intenſities of both were the ſame, yet the other effects were much greater in the pile with large paſteboards; for it gave very brilliant ſparks, which began with the fifth pair from the bottom, while the other pile ſcarcely emitted any that were perceptible. On making the paſteboards ſomewhat larger than the metallic plates, the effect was by no means increaſed, but rather diminiſhed. It was found neceſſary, likewiſe, that the paſteboards in the large pile ſhould be well wetted.

Effects of different ſized paſteboards.

Theſe ſhould not be too large:

and ſhould be well wetted.

There is another apparent anomaly, however, in the effects of piles differing in diameter: though the action of the large pile is ſo much more powerful in fuſing metals, not the leaſt difference can be perceived in the ſhocks they give: and this Dr. Van Marum confeſſes is not accounted for by his hypotheſis.

Anomaly not accounted for by the hypotheſis.

Some farther experiments were made reſpecting the efficacy of the ſolution of muriate of ammonia, which was found to be preferable to common water, or to a ſolution of muriate of ſoda, for moiſtening the cloth or paſteboard. A pile of twenty pairs with the ammoniac ſolution fuſed into globules four inches of wire, No. 16; while that with ſolution of common ſalt did not fuſe or even redden a ſingle line, and that with ſimple water ſhewed ſtill leſs effect with regard to the ſparks it produced.

Solution of muriate of ammonia preferable for moiſtening the cloths.

As ſeveral authors have aſcribed the greater effect produced by ſaline ſolutions to the oxidation of the metals, many experiments were tried, with a view to aſcertain this point, by

Not becauſe it oxidates the metals, for it is ſaid to be ſupe-

rior to the more powerful acids.

by moistening the pasteboards with sulphuric, nitric, and muriatic acids, both in a concentrated state, and more or less diluted. The particulars are not given, but the results are said not to have favoured the supposition, on a comparison of the intensities, shocks, or sparks; all of which were stronger when muriate of ammonia was employed, than when nitric acid was used, either concentrated or diluted, by which the two metals was very speedily oxidated. As nothing is said here on the fusion of wire, this does not appear to have been tried.

Ammonia alone not equal to the muriate.

Ammonia alone being used to moisten the pasteboards, the effects produced were all less than with its muriate.

Effects of the pile in the air, in vacuo, in carbonated hidrogen gas, and in azote gas, equal:

Experiments were likewise made in vacuo, and in different airs. The apparatus employed very readily produces a vacuum in which the barometer falls to below one line; but in this instance the vapor produced by the water of the pile kept the quicksilver at the height of five lines. Between the effects produced in the open air, in this vacuum, in carbonated hidrogen gas, and in azote gas, no difference could be discovered.

but in pure oxigen gas much increased.

On the introduction of pure oxigen gas, however, the sparks were much larger, more brilliant, and easier to be obtained: but on making a vacuum after this, the shocks were feebler, and the sparks smaller, than they had been in any of the former instances. Oxigen gas being introduced a second time, its effects were as powerful as before: the cylinder being emptied again, the effects of the pile were reduced as much as before: and on letting in atmospheric air they were restored to the same degree nearly as in the former experiments.

A pile with potash was very strong, but shewed no oxidation.

An experiment of the Doctor was directed to ascertain the presence of oxidation in the use of a very concentrated solution of potash in a pile of twelve pairs of five inches square, which produced much more effect than another similar pile with water only. But on separating the pile neither the copper nor the zinc had suffered any injury in their polish. The Doctor does does not say how long the pile stood together.

Very large pile of five inches square.

Lastly, The galvanic apparatus was increased to 200 pairs of zinc and copper in six connected piles of five inches square each. With this he fused an iron wire, No. 16, of twenty-three inches entirely into globules, and ignited the whole of another piece of thirty-three inches.

The

The shock was tried of a column of twenty pairs of copper and zinc of one inch and a half, and of another of the same number of plates of five inches; and also of another of ten inches square; but not the least difference could be perceived. The last mentioned pile (which, as well as the others, was moistened with muriate of ammonia), fused five inches of wire; and the other of five inches square fused four inches. Whence it seems to follow that the effects of these piles, as to the fusion of wire, do not increase in the proportion of their surfaces, but in a less ratio.

The shocks not at all increased by the increase of surface.

Fusion of wire does not increase as the surface, but in a less ratio.

IV.

A Reply to Mr. CRUIKSHANK'S *Observations in Defence of the New System of Chemistry, in the Philosophical Journal* *. *By* JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c. Communicated by the Author.*

HAVING proposed to philosophers the re-consideration of the doctrine of *phlogiston*, which for some time has been almost universally exploded, I am happy to find so truly ingenious and candid a person as Mr. Cruikshank has given some attention to the subject. That experiment of mine which he particularly examines, is that in which I procured a very large quantity of heavy inflammable air from finery cinder and charcoal, both previously exposed to such a degree of heat, as would have expelled from them all the air that mere heat could expel. This I ascribed to the water in the finery cinder uniting with phlogiston from the charcoal. Mr. Berthollet thinks, that this inflammable air comes from the decomposition of the water contained in the charcoal, and Dr. Woodhouse, from that which he allows to be retained in the finery cinder. But Mr. Cruikshank, not satisfied, I presume, with either of these hypotheses, has suggested a very different one, for he asserts, that water is not at all necessary to the production of this inflammable air, maintaining that metals, and their calces, in a very high temperature, have the power of decomposing fixed air, and in this case the fixed air must be formed from the oxigen in the finery cinder, and the carbon in the charcoal.

Short preface and recapitulation.

The heavy inflammable air from finery cinder and charcoal,

ascribed to water either simple or decomposed;

but by Mr. Cruikshank to a decomposition of carbonic acid.

* Vol. V. Quarto Series, page 1.

After

His experiments with metallic oxides and charcoal, in proof, are inferred by Dr. P. to prove that the oxides contain water.

After repeating my experiment, which he found to be juſt, Mr. Cruikſhank did the ſame with the calces of other metals, particularly thoſe of zinc, copper, lead, and manganeſe, and then concludes, p. 4, that in all theſe caſes "the air muſt "come from the partial decompoſition of the carbonic acid by "the calx when raiſed to a high temperature." But the inference that I think is more naturally drawn from them is, that all theſe calces contain much water, and little or nothing elſe. This I have ſhewn to be the caſe with reſpect to ſeveral of them, eſpecially that of zinc; though I doubt not but that ſome ſmall portion of oxigen may be contained in them all. Indeed, we cannot abſolutely ſay, that any ſubſtance whatever can be wholly expelled from any other, with which it has been chemically combined by any proceſs.

Iron or its oxide heated by the ſolar focus in carbonic acid is ſtated to have produced no decompoſition; for though part was rendered immiſcible with water by thus heating either an oxide or a piece of earthen crucible in it; yet there was no addition of oxigen, but only azote.

Before Mr. Cruikſhank admitted that iron, or its calx, when raiſed to a high temperature, can decompoſe carbonic acid (*i. e.* fixed air) in this experiment, he ſhould have tried whether it would do it in any other. If in any caſe, I ſhould think it would do it when it was heated in this air by a burning lens, by which a greater heat may be produced than in any open fire. But this I found not to be the caſe either with iron, or this calx of it. In the laſt ſummer I went through a courſe of experiments with this view; but I always found fixed air not to be decompoſed by this means. Though I found that a portion of this air, and alſo of all the other kinds that are readily imbibed by water, was rendered immiſcible in water by means of heat reflected either from a calx of any metal, a piece of earthen crucible, or any other ſubſtance, on which I threw the focus of the lens when it was ſurrounded by this kind of air confined by mercury or water: This, however, was no decompoſition of the air, as there was no oxigen found in it after the proceſs. The addition of permanent air was always phlogiſticated.

Mr. C. produced heavy inflammable gas from iron filings and chalk heated;

Mr. Cruikſhank thought, that if this heavy inflammable air came from the decompoſition of the carbonic acid by the iron, he ſhould ſucceed better, p. 4. by employing iron filings in the place of finery cinder, as they would have a greater affinity with oxigen; and with this view he heated them together a quantity of common chalk, previouſly expoſed to a low heat, for ten minutes. From this mixture he procured a *great quantity of air*, and he thought that the acid (*i. e*, fixed

air

air from the chalk) was decompofed by the iron; whereas when he ufed well burned lime he got little or no air. What I infer from this experiment is, that the chalk, not being perfectly calcined, contained fome water, as well as fixed air, and that this water uniting with the phlogifton of the iron formed the inflammable air that he found. Water I fuppofe to be the bafis of all the kinds of air, and many fubftances retain it in any degree of heat. Chalk I have found to do it after long expofure to the heat of a fmith's forge.

which the Dr. does not infer to be carbonic acid decompofed; but water with phlogifton from the iron.

Admitting the fixed air procured in the experiment with the finery cinder and charcoal to come in part from the oxigen in the finery cinder, how is this oxigen to be expelled from the calx, fince heat will not do it? And there is no inftance, I believe, in chemiftry, in which when heat alone will not expel any conftituent part of a fubftance, it can be effected without the aid of an affinity, in confequence of which fome other fubftance takes its place. But here, according to the new theory, nothing is fuppofed to take the place of the oxigen in the finery cinder. It takes nothing from the charcoal, but the iron is revived by the mere expulfion of the oxigen.

If the carbonic acid, from finery cinder and charcoal, derive its oxigen from the oxide, this principle muft, it is ftated, be transferred contrary to the laws of affinity.

Mr. Cruikfhank lays great ftrefs on the difference that he found in the air that he procured in thefe proceffes from that which is got from charcoal and water. But I have obferved, that there is a confiderable difference in the qualities of heavy inflammable air, not only according to the fubftance from which it is procured, but in the fucceffive ftages of the fame procefs. He will find that I have examined this kind of air as procured from a great variety of fubftances, made to pafs in the form of vapour through hot earthen tubes, and in various other ways, and have given the analyfis of them. I always found that the firft portions from charcoal were loaded with fixed air, but that in the courfe of the procefs this difappeared, the air burning with a lambent flame, and that towards the end it approached to the explofive kinds, as obtained from metals by acids.

The difference between this gas and that from mere water and charcoal, ftated to afford no conclufive argument.

I alfo found that more or lefs fixed air is procured by the decompofition of heavy inflammable air by means of dephlogifticated air; and though the air procured from finery cinder and charcoal fhewed no fign of its containing any mixture of fixed air, nothing of the kind being difcoverable by lime water;

Oxigen combined with heavy inflammable air affords carbonic acid, in greater quantity than the latter; which

Is inferred to have afforded phlogiston.

water; yet when it was decompofed I found much more than the weight of the air; fo that it could not have been previoufly contained in it in a ftate of folution, but muft have been formed by the union of the oxigen in the dephlogifticated air, and the phlogifton in the inflammable air.

Charcoal fuppofed to contain carbon and phlogifton.

That charcoal uniting with water fhould give fixed as well as inflammable air, I account for by fuppofing, what is by no means improbable, that this fubftance contains the elements of both the kinds of air, and that they want nothing but water to enable them to take the form of air.

Conclufion.

I hope that Mr. Cruikfhank, with the fame candour with which he has began this difcuffion, wlll re-confider his hypothefis, and extend his examination to my other arguments in defence of the doctrine of phlogifton, and againft the decompofition of water. Nothing but free difcuffion is neceffary to the difcovery of truth, and it is defirable that error fhould be detected as early as may be, efpecially if the confequence of its reception be extenfive and important.

Your's fincerely,

J. PRIESTLEY.

V.

A Statement of the Experiments made by the Rev. ABRAHAM BENNET, *F. R. S. on the Electricity produced by the contact of Metals previous to the Year* 1789, *and alfo of thofe made by Mr.* TIBERIUS CAVALLO, *F. R. S. previous to the Year* 1795, *to which Allufion was made at Page* 144 *of this Journal.* W. N.

Short ftatement of Bennet's experiments, and why they are here repeated.

SOMETIME previous to the period firft mentioned in the title to the prefent fketch, Mr. Bennet had remarkably increafed our power of meafuring fmall intenfities of fingle electricity by the application of gold leaf to the bottle electrometer of Cavallo, and by the procefs known by the name of doubling; an outline of the hiftory of which may be feen at page 396 of the firft volume of the Philofophical Journal, in quarto. His *New Experiments on Electricity,* which is a thin quarto of 141 pages, were publifhed in 1789 by fubfcription; and may perhaps have been lefs univerfally diffufed in the

fcientific

ſcientific world, than if the book had appeared in the uſual mode of publication. At all events, as the work is now ſcarce, and the ſubject of ſome intereſt ſo far as it may be thought connected with the theory of galvaniſm, I think it will not be unacceptable to my readers to relate what he did at that early period. The ſame reaſons will in part apply in favour of a conciſe account of the experiments of Mr. Cavallo.

Soon after the publication of Bennet's doubler in the Philoſophical Tranſactions for 1787, he found that the inſtrument produced electricity without previous communication, and that it always retained that property, whatever care might be taken to deprive it of any adhering charge. When he was afterwards engaged in a courſe of experiments facilitated by the mechaniſm I applied to the doubler in 1788, he found that a very great portion of this adhering electricity might be removed, by turning the handle of the doubler a conſiderable number of times, while all the plates were connected with the earth, and that by virtue of this proviſion the inſtrument might be truſted to indicate the nature of communicated electricity, to a degree of accuracy far exceeding that which could be afforded by any ſimpler inſtrument.

Uncertainty of the electrical doubler; from ſpontaneous electricity,

removed by working its parts uninſulated.

By reaſoning upon this phenomenon, he was induced to conjecture, that this ſpontaneous electricity was not owing to accidental friction, but to what he called the increaſed capacity of approximating parallel plates, which might attract and retain a charge, though neither of them were inſulated. The experiments he made in ſupport or proof of this hypotheſis were the following:

Capacity of bodies for electricity ſuppoſed to increaſe by approximation.

He repeatedly tried the effect of depriving the doubler of ſpontaneous electricity, by turning the revolving plate forty times, with braſs wires hooked to all the plates, and he found that if he took off the wires while the revolving plate ſtood at a diſtance from the two ſtationary plates, it was more completely deprived of electricity, than if the wires were taken off when the revolving plate ſtood parallel to the plate A. That is to ſay, it required 21 revolutions to exhibit ſpontaneous electricity after the proviſion in the firſt caſe, and 16 revolutions in the ſecond caſe. Whence it ſeemed to him, that the two plates ſtanding parallel to each other, had by an increaſe of capacity acquired a certain ſmall charge, which ſooner

Experiments. The doubler ſooner exhibited electricity when cleared and left with its plates oppoſite, than when at a diſtance from each other.

fooner rofe by doubling to a perceptible quantity, than that electricity which they might have poffeffed in the firft cafe.

An infulated copper plate placed in contact with a table, with a large approximating furface became negative.

But to make the experiment in a more direct manner, ufing the doubler only as an inftrument of admeafurement, he took a copper plate thirteen inches in diameter, having its furface rather convex, furnifhed with an infulating handle of oiled glafs four inches long, and baked till the whole was well hardened. One end of this glafs was fixed into a copper focket in the middle of the plate, and the other end into wood, that it might not be neceffary to touch the electric part of the handle. This copper plate was placed flat upon a mahogany table, and the doubler being deprived of its electricity, the revolving plate B was placed parallel to A, fo that B was connected with the earth; then the copper plate was lifted up by its infulating handle, and applied to the plate A. And laftly, the plate B being revolved only five times, caufed the gold leaf of the electrometer to diverge negatively to the diftance of a quarter of an inch.

The fame refult when a furface of water was ufed inftead of the table.

It might in this inftance eafily have been ftated, that the contact of the copper plate with the mahogany table, did in fact produce electricity by excitation or friction. But to obviate this, he firft repeated the experiment, by touching the copper plate with the point of a needle, and then applying it to the doubler as before, and he found that the inftrument did not produce its fpontaneous electricity in lefs than 15 revolutions; after which touching the copper plate again with the needle, he applied it to the furface of fome water contained in a large difh, fo that its convex part touched the fluid; and then lifting it up he applied it to the doubler, and electricity was communicated, fufficient to caufe the gold leaf to diverge negatively at five revolutions, as in the former experiment.

When the touch was reverfed the oppofite power was obtained.

The laft experiment with the water was repeated, with this difference only, that the application of the copper plate was made to the revolving plate of the doubler, inftead of the plate A, the revolving plate being moved a little paft its contact with the ball, and the plate A being at that time made to communicate with the earth by the brafs wire; five revolutions in this cafe produced electricity as before, but the electricity of the plate A was now pofitive, as might be expected in confequence of the negative charge having been communicated to B inftead of A.

The

The foregoing experiments were frequently repeated, but as the charge had hitherto been negative, he was defirous of knowing, whether a variation of the nature of the furface might not alter it. He therefore covered the furface of the copper plate with a mixture of gum water and minium, and alfo with gum water and wheat flour, and he found that this fubftance when dried upon the furface of the copper, changed the nature of the charge communicated to the plate to which it was applied.

When the plate was covered with gum water, &c. and dried, its electricity became pofitive.

Laftly, to render what he called the electricity of the approximating plates more confpicuoufly fenfible, he ground a brafs plate three inches diameter with emery, till it would adhere to the furface of a piece of black marble. This plate and marble therefore conftituted a condenfer in its original ftate. The marble being moderately warmed, he preffed the brafs plate upon its furface with the point of a brafs wire; then lifting it up with its infulating handle he applied it to the cap of the electrometer, which caufed the gold leaf to ftrike the fide negatively.

The condenfer affords electricity.

Hence he thinks it muft appear evident, from the precautions and experiments he has ftated, and from the known laws of electricity. 1. That the doubler may be deprived of accidental or communicated electricity. 2. That the principal caufe of its fpontaneous charge, is the attraction of electricity by the approximation of its parallel plates. 3. That this charge may be pofitive or negative, according as the plates or touching wires are compofed of fubftances which have a greater or lefs adhefive affinity with the electric fluid. 4. That the caufes of fpontaneous electricity are common to the condenfer, both in its original and improved ftate, and the doubler, and equal in them all, as far as they are equal in their dimenfions and powers. 5. That fince the doubler may be compofed of very fmall plates, and yet its power be equal to that of a very large condenfer, its fpontaneous electricity will be more eafily overcome by a communicated charge than that of a condenfer of equal power, and therefore experiments performed with it will be lefs liable to equivocal refults.

Enumeration of refults. 1. The doubler cleared of electricity. 2. Caufe of fpontaneous charge. 3. and its nature. 4. Common to all condenfers. 5. The doubler preferable.

This author proceeded to make other experiments, on the fo termed adhefive electricity of metals, and other conducting fubftances. He deprived the doubler of its fpontaneous charge, and placed the two plates A and B oppofite

Other experiments of adhefive electricity; with iron and fteel;

each

each other, but fo that B was not connected with the earth. He then touched the plate A with the blade of a knife, and the plate B at the fame time with the point of a foftened iron wire. With fixteen revolutions the gold leaf diverged about one third of an inch pofitively. The doubler was then again deprived of electricity, and the revolving plate B placed as in the laft experiment. The knife was applied to B inftead of A, and the foft iron wire to A inftead of B, which opened the gold leaf negatively at fifteen revolutions. Thefe experiments were repeated very often, and the electricity changed each time; being always pofitive in the plate touched with the knife. He remarks, that it would appear incredible, that fo minute a difference of adhefive electricity as that which might be fuppofed between two metals fo nearly alike as hardened fteel and foft iron could be diftinguifhed, had not the frequent repetition of experiments confirmed it.

with other fubftances.

He proceeded to make fimilar experiments, which are tabulated, of different fubftances, namely, lead ore and lead; lead and iron wire; lead ore and iron wire; tin foil and iron wire; zinc and iron wire. All the experiments having been made by double contact and alternate application of the oppofite fubftances to the plates A and B, he alfo by fingle contact determined whether the adhefive electricity, or to ftate the fact more unexceptionably, the electricity produced by each fubftance in the doubler, was pofitive or negative. I have thought it lefs immediately neceffary to ftate the particulars of thefe experiments, becaufe I prefume that the fubftances were held in the hand, and from a variety of facts, I think we are juftified in concluding that friction, or a contact equivalent to friction, between the human fkin and the fubftance thus held, will produce electricity more than fufficient to render it unneceffary to look for any other caufe, or at leaft quite fufficient to render our inveftigations uncertain when fo conducted. That is to fay, I think the refults will rather confift of determinations of the kind of electricity produced by friction of the hand upon the feveral fubftances, than any new or peculiar affection of the fubftances themfelves.

Objection from the effect of friction by the hand.

Among other interefting obfervations, for which I muft refer the reader to the work itfelf, he gives a fimple theory of the excitation of glafs, and other electrics ufed in the conftruction of our machines. He remarks, that when the filk

Theory of electric excitation.

3 flap

flap is rubbed by the revolving glafs cylinder it is brought into clofe contact; and the electricity adhering more forcibly to the glafs, is carried forward into the open air, which air not having been rendered negative like the filk, does not counterbalance the furface of the glafs, and therefore its capacity being leffened, it emits the charge it had juft abforbed. And he thinks the amalgamated cufhion affifts the procefs, by bringing a furface of a conducting quality, and in connection with the earth into clofer contact.

Cavallo's experiments of electricity afforded by the fall of conducting bodies upon a tin plate.

The experiments of Mr. Cavallo were made by letting fubftances fall *out of his hand for the moft part* upon an infulated plate of tin, whence they were fhook off upon a table, or upon a chair; from which he again took the fubftance up, and let it fall upon the tin plate to the number of times required. After thefe reiterations he applied his tin plate to the plate of his multiplier. In his experiments a piece of zinc little more than half an ounce, was dropped ten times fucceffively upon the plate, which laft, as appeared by the fubfequent operations of the multiplier, was electrified negatively. Another piece dropped ten times produced the fame effect. Zinc was heated to 110 degrees, and one repetition of the experiment produced the fame effect, but ftronger. A fhilling, an half crown, a new guinea, a piece of copper, a piece of malleable platina, with like manipulation, produced fimilar effects, but differing in degree. Platina produced very little electricity; but when heated it was found to produce an effect contrary to the preceding; that is to fay, it electrified the tin plate pofitively. A piece of lead appeared to produce negative electricity in the tin; but when hot pofitive. A piece of iron afforded very equivocal refults. A piece of grain tin afforded negative electricity, as well when heated as when cold.— When the cold tin was dropped from a pair of iron tongs, and let fall from the tin plate upon a chair, whence it was picked up by means of the fame iron tongs, it produced weak pofitive electricity. Tin when heated afforded electricity of the fame quality, but greater in quantity. Mr. Cavallo alternately ufed the hand and the iron tongs with refults generally like the preceding, that is to fay, negative electricity in the tin plate, when the grain tin was dropped from the hand, and pofitive when it fell from the tongs. Bifmuth produced pofitive electricity; but when the bifmuth was made very hot, the

Cavallo's experiments of electricity afforded by the fall of conducting bodies upon a tin plate.

the electricity of the tin plate was negative. These results were the same, when the bismuth was cast into a smooth flat piece, instead of being broken from a lump. When the iron tongs were used with the bismuth, the tin plate became negative, contrary to what it was when the hand was used.

An insulated silver spoon was substituted in the place of the tin plate, and in this zinc dropped from the hand produced negative electricity, which was stronger when the zinc was heated. The kind of electricity was not changed when the spoon was heated, and the zinc cold, but its degree was much less. Mr. Cavallo considers it as a very extraordinary fact, that the experiments varied as to the intensity of the electricity very much on different days, and is inclined to ascribe this difference to the disposition of the atmosphere.

In order to discover the source of the electricity produced in these experiments, he repeated them in a great variety of ways, namely, instead of the hand, he dropped the zinc from a tin plate held with one hand into a spoon, and from the latter back upon the former. He performed this operation with both the tin plate and the silver spoon insulated. He likewise tied a silk thread to the zinc, and holding the other extremity of the thread in one hand, struck the zinc repeatedly against the spoon; but in those cases very seldom any electricity was manifested, except when the weather, and every other circumstance was very favourable, and then the electricity could with difficulty be manifested; yet when the tin plate was held in the hand, and the zinc thrown from it into an insulated spoon, some electricity was more frequently produced than in the other two cases.

Whether animal electricity can be deduced from the preceding facts? &c.

After a careful review of his experiments, he doubts whether the phenomena of animal electricity can be attributed to the cause supposed to operate in them. For as he remarks, the action of metallic bodies produces the same effects, with hardly any observable difference, upon prepared animal limbs; whereas the effects in his experiments were fluctuating, and differed considerably in zinc and bismuth, which nevertheless do not excite the animal electricity more powerfully than zinc and silver, or zinc and gold. He also remarks, that he found by experiments with minute quantities of electricity applied to the prepared limbs, that they were not excited when these quantities were nevertheless very much greater than what was produced

produced in his other experiments. But on the whole, he thinks they feem to eftablifh, 1. That the contact of one metallic fubftance with another generally produces electricity. 2. That the quantity and quality of the electricity fo produced, is various, according to many circumftances which feem to occur in the products of it, or in a great meafure to influence it. 3. And that thefe circumftances are, the various nature of the metallic fubftances, their various degrees of heat, the ftate of the atmofphere, the hand of the operator, &c. each of which caufes has its fhare in the refult.

VI.

On the Formation of Cryftals, defcribing a Method of producing them large and regular. By CIT. LE BLANC *.

On the caufes of cryftallization.

IT had long fince been remarked, that the fame falt is fufceptible of cryftallizing under feveral different forms. C. Hauy has demonftrated, that all thefe fecondary forms are owing to different arrangements of the fame integrant molecule; he has fhewn that thefe effects are not the effect of what is termed chance, but that they proceed from laws fufficiently fimple, which may eafily be determined. Here he has ftopped; he has not yet thought proper to publifh the ideas which he has given in his courfe [of lectures] relative to the caufes which difpofe the integrant molecules to follow fuch or fuch a law in their mutual arrangement. Thefe are the caufes which Cit. Le Blanc inveftigates in his obfervations on the growth of cryftals. He has been long engaged in thefe inquiries, and the memoir which he has read to the Inftitute is a confimation and fequel of that which he read to the Academy of Sciences, of which an extract was inferted in the Journal de Phyfique, November 1788, p. 374. He has difcovered, by his perfevering and ingenious obfervations, that we may confiderably vary both the bulk and form of the cryftals at pleafure, by caufing them to be formed and to grow under certain circumftances, and he has long fince en-

Cryftals of extraordinary fize and beauty.

* Communicated to the French National Inftitute, and abridged in the Bulletin des Sciences, whence the above is tranflated, No. 50, An. 10.

riched

riched the collections with cryſtals of alum, ſea-ſalt, ſulphate of copper, &c. of an extraordinary ſize and beauty: he now publiſhes the means which he has employed.

How made.

Flat-bottomed veſſels of glaſs or porcelain, are the beſt for obtaining beautiful ſingle cryſtals. The ſolutions ought to be brought to the point of cryſtallization. They firſt yield cryſtals that are very ſmall. Amongſt theſe ſmall cryſtals, which Cit. Le Blanc calls embryos, a ſelection is made of the neateſt, in order to promote their growth, or as C. Le Blanc terms it, to *nurſe (elever)* them. The liquor is decanted in order to purify it, and the ſmall cryſtals that have been ſelected are diſtributed in it, and carefully turned every day. Amongſt theſe cryſtals a ſecond ſelection is made, in order ſeparately to nurſe thoſe of which we wiſh either to augment the volume, or change the form.

Embryo cryſtals nurſed or reared,

by ſelecting the placing them in the mother water.

In order to make them grow without irregularity, they muſt be placed in the mother-water of a ſolution that has afforded a confuſed cryſtallization Care muſt be taken to turn them often, and to give them freſh ſupplies of mother-water in proportion as they increaſe in growth. In this manner they may be brought to a conſiderable volume.

If they be left too long in a ſolution in which they have acquired their full growth, they diminiſh inſtead of increaſing in ſize, and it is obſerved that this decreaſe takes place at the angles and edges, ſo as to leave ſtriæ viſible, which indicate the direction of the ranges of ſubjacent molecules.—The poſition of the cryſtals in the ſolution influences their form: this is particularly remarkable in the priſmatic cryſtals: they grow in length when they are laid upon one of their ſides, and in breadth when they are placed upon their baſe.

They may be made to grow either in length or breadth.

Secondary forms.

Cit. Le Blanc having changed octahedral alum into cubic alum, by placing an octahedral cryſtal in a ſolution of alum ſaturated with its earth, which gives the cube, infers from thence that frequently the ſecondary forms are owing to differences in the proportion of the principles *.

A curious

* It appears to us that this fact cannot lead, more than any other, to ſuch an inference. According to the experiments of Cit. Vauquelin, the alumine in exceſs is mixed with the ſulphate of alumine, but it is not combined with it; for ſimple ſolution in water is ſufficient

A curious obfervation of Cit. Le Blanc, which has already been recorded in the Journal de Phyfique, proves that the fame folution left to itfelf, is not equally faturated in all its parts. If we fufpend cryftals at different heights in a folution, the loweft cryftals increafe more rapidly than the higher ones; and it fometimes happens that thefe diffolve whilft the lower ftill continue to grow. Citizen Le Blanc adverts to the analogy that fubfifts between this obfervation, and that of the more complete faturation of the water of the fea at great depths.

Solutions are more faturated at greater depths.

Cit. Le Blanc informs us, that by adding fulphate of copper which cryftallizes in oblique prifms, to fulphate of copper which cryftallizes in the octahedral form, rhomboids are conftantly obtained *.

VII.

Accounts of the New Planet CERES †.

ON the 4th of February a letter from Dr. Mafkelyne was read before the Royal Society, announcing that he had obferved the new planet of Mr. Piazzi paffing the meridian between three and four o'clock in the morning, having about 138° 43′ right afcenfion, and 12° 38′ north declination, appearing like a ftar of the eighth magnitude.

Letters to the Royal Society, announcing obfervations of the new planet by Dr. Mafkelyne, Mr. von Zach, and A. Aubert, Efq.

Another letter from Mr. von Zach was read, informing the Society that he had obferved this planet at Seeberg on the 7th of December, within half a degree of the place before

cient to feparate it. Thefe cryftals are accordingly opaque: befides, the fame chemift has obtained cubic and tranfparent cryftals from acidulous fulphate of alumine. (Note of the Editors).

* We muft obferve that the primitive form of the fulphate of iron is the rhomboid, and that the irregular octahedron which it prefents is a fecondary form. Cit. Hauy has examined one of thefe cryftals refulting from a mixture of a folution of fulphate of copper with a folution of fulphate of iron. The rhomboid which he examined differed in no refpect from the primitive rhomboid of the fulphate of iron. (Note of the Editors.)

† This whole article is taken verbatim from the Journal of the Royal Inftitution.

determined in his journal. Mr. Olbers ſaw it at Bremen on the 2nd of January. With a power of above 120 it preſented no obſervable diſc.

On the 11th a ſecond letter from the Aſtronomer Royal informed the Society that he had repeated his obſervation of the new planet, ſo as fully to aſcertain its motion. It appeared to have a viſible diſc when on the meridian, and viewed with a power of 50. When the air was very clear the diſc was round and well defined, but ſomewhat ſmaller than that of the 34th of Virgo, a ſtar of the 6th magnitude near it. Dr. Maſkelyne obſerves, that the ſmallneſs and roundneſs of the appearance of the diſc of the fixed ſtars is a good criterion of the clearneſs of the air.

Another letter from Alexander Aubert, Eſq. F. R. S. was alſo read. Mr. Aubert diſcovered the planet Ceres on Sunday morning, having about 188° 41 right aſcenſion, and 13° declination, its motion at preſent being retrograde.

Hiſtory of the planet as diſcovered by Piazzi; obſerved by Olbers, and conſidered by Oriani, Zach, Bode, Lalande, and Burckhardt.

Citizen Burckhardt in the Moniteur, 4 *Pluv. An.* 10. *No.* 124. gives the following account. The planet which Mr. Piazzi diſcovered at Palermo the firſt of January 1801, was again ſeen the firſt of January 1802, by Mr. Olbers, at Bremen, nearly in the place where it was expected from the calculations of Mr. von Zach. The 2nd January 1802, at 18^h. 58′ 36″, mean time, at Bremen, its right aſcenſion was 185° 9′, and its declination 11° 9′ north, in the wing of Virgo, near a ſtar of which Lalande had given the poſition, in the Connaiſſance des Tems, Year 9, p. 254. The 5th January, at 17^h. 36′, its right aſcenſion was 185° 43′, and its declination 11° 8′, nearly. It appears as a ſtar of the ninth magnitude, but it will become more conſpicuous. With a teleſcope magnifying 106 times, it cannot be diſtinguiſhed from a ſmall ſtar.

The 1ſt January it fortunately made a right angled triangle with two ſmall ſtars mentioned in Lalande's Hiſtoire Céleſte; the following day the form of the triangle was changed, and by means of this change the planet was recogniſed. It will be on the parallel of the 20th of Virgo.

The elements of this planet have occupied ſeveral aſtronomers. Meſſrs. Oriani, Zach, and Bode, had ſuſpected at once that it was a planet, becauſe it had been obſerved ſtationary, and without nebuloſity. But having received only two complete obſervations, they had not been able to confirm their

their ſuſpicions. Some time afterwards, Mr. Lalande firſt obtained a complete copy of the obſervations of Mr. Piazzi, who could not refuſe them to one, under whom he had ſo long applied to the ſtudy of aſtronomy. By means of theſe obſervations, I was the firſt that demonſtrated, in a memoir preſented to the National Inſtitute, that there was no parabolic orbit that could agree with the obſervations, although confined to an arc of 10 degrees. I gave at the ſame time the elements of a circular and of an elliptic orbit, and I ſhowed the great uncertainty that neceſſarily remains when the elements are deduced from ſo ſmall an arc.

Having received a more exact copy of theſe obſervations, Mr. Olbers endeavoured to determine from them the elements of an elliptic orbit; but he found ſo much uncertainty that he was obliged to prefer a circular orbit, ſince he thought it impoſſible to determine if the planet was near its aphelion, or its perihelion. I had proceeded on the former ſuppoſition; Mr. Gauſs preferred the latter, and endeavoured at the ſame time to accommodate his calculations to all the obſervations of Mr. Piazzi: and this he performed with a difference of only a few ſeconds. Theſe are his elements: — Elements of its orbit

Epoch of 1801 - -	2s	[17 ?]°	36′	34″
Aphelion - - -	10	26	27	38
Node - - - -	2	21	0	44
Inclination - - -		10	36	57
Greateſt equation of the centre		9	27	41
Heliocentric and tropical diurnal motion			12	50.914

Mean diſtance 2.7673. Eccentricity .0825.

Revolution 1681 days, or 4 years 7 months.

I had found the revolution 5 months and a half ſhorter.

According to M. Lalande's calculations, Mr. Gauſs's elements give the longitude greater by a degree than Mr. Olbers's obſervation: according to Mr. von. Zach, my elements give it four degrees leſs, and Piazzi's, ten degrees leſs than the obſervation.

The idea of ſearching for this planet among the immenſe collection of obſervations of the Hiſtoire Céleſte Françaiſe, could not fail to preſent itſelf to all thoſe who have attended to the ſubject: but it was impoſſible to undertake the inquiry with any hopes of ſucceſs, before the elements were corrected by new obſervations. I ſhall now apply to it without delay. — give the power of ſearching for the planet in the collections of obſervations.

 Mr.

Mr. Piazzi has named his planet Ceres Ferdinandia. Lalande proposes to call it Piazzi.

Some account of M. Piazzi.

Mr. Piazzi was born at Ponte in the Valteline; and was professor at Malta, and at Palermo. When an observatory was about to be established at Palermo, in 1787, he came to Paris; he then went to London, where he procured some excellent instruments: and he has already published two volumes of valuable observations: he is now preparing to measure a degree in Sicily, and Mr. Lalande has already sent him instruments for this purpose.

Extract from Bode's Kurzer Entwurf der Astronomischen Wissenschaften. Berlin. 1794. § 387.

Numerical analogy by which *Bode* suspected the existence of a planet between Mars and Jupiter seven years ago.

Is it probable that Uranus, or the Georgian planet, is really situated at the utmost limit of our solar world? This appears to be very doubtful, considering the immense space interposed between it and the nearest fixed stars. Other planets perhaps may be still more remotely situated, and may perform their revolutions unseen by human eyes. We can scarcely suppose that any planet exists nearer to the sun than Mercury: but considering the proportions of the distances of the planets from the sun, we observe between Mars and Jupiter, a distance far greater than a comparison of the other distances would lead us to expect, and this space may perhaps be occupied by a planet yet unknown.

This appears to follow from a certain proportion which we find among the distances of the seven planets already known. Calling the distance of Saturn 100, that of Mercury will be nearly 4, of Venus $4 + 3 = 7$; of the Earth $4 + 2 \times 3 = 10$; of Mars $4 + 4 \times 4 = 16$; we then want a planet at the distance $4 + 8 \times 3 = 28$: the distance of Jupiter is $4 + 16 \times 3 = 52$; of Saturn $4 + 32 \times 3 = 100$; and of the Georgian planet $4 + 64 \times 3 = 196$.

Observation; by whom?

It will, however, still be doubted by many if the conjecture quoted from Professor Bode can be thought to have been probable at the time that it was made. Calling the distance of the Earth 10, the real proportional distances are, in the nearest units, Mercury 4, Venus 7, Earth 10, Mars 15, (Ceres 28,) Jupiter 52, Saturn 95, Georgium 92. Instead of Mars 16, Saturn 100, and Georgium 196.

Letters

Letters from Sir Henry Englefield, Bart. F. R. S. to Thomas Young, M. D. F. R. S. on the Planet Ceres.

SIR, Blackheath, *Friday.*

I have feen the new planet twice, on Sunday night, and again laft night. It is juft vifible to a common night-glafs. With a power of 90 in my great telefcope it was lefs bright than the 34 Virginis, near which it is. With a power of 200 no difc is vifible, and with 300 I can fcarcely fay that it has a fenfible diameter, more than what arifes from irradiation, for fmall ftars feen with fuch powers always appear dilated. Obfervations of the new planet by Sir H. Englefield, Bart.

I looked at the Georgian foon after the new planet, but clouds came on, and I did not try 300 on it laft night. With 200 the Georgian is, I am fure, the brighter, and it was a very much more vifible object in the night-glafs.

Sunday.

Laft night I again faw the new planet, and obferved it with a power of 400. With this great power it feemed to have an apparent magnitude, but was extremely fmall, faint, and ill defined. I then turned the telefcope to the Georgian (which as you know is very near), and the fuperiority in fize and brightnefs was very ftriking. The Georgian was not well defined, but I am fure it was full four times the diameter of the new planet, and much brighter in proportion to the different fize. Indeed the brightnefs of the Georgian is very furprifing, its vaft diftance from the fun being confidered. I really think that the diameter of the new planet cannot exceed a fecond; and it is of a very faint light even for that diameter. I looked then at the double ftar gamma Virginis, and faw the two ftars diftant from each other full three times their apparent diameter, a proof of the good adjuftment and high power of my telefcope. Its diameter very fmall.

I am, &c.

H. E.

VIII. *Obfervations*

VIII.

Obſervations and Experiments relating to the Pile of VOLTA. *In a Letter from* JOSEPH PRIESTLEY, *LL.D. F.R.S.*

To Mr. NICHOLSON.

DEAR SIR,

Galvanic pile.

HAVING been favoured by Mr. Weatherby Phipſon, a young man of Birmingham, with an excellent apparatus for repeating the experiments on the *pile of Volta*, conſiſting of ſixty plates of copper coated with ſilver, and as many thin rolled plates of zinc, which is a valuable improvement of his own, I have had great ſatisfaction in obſerving the reſults; and though, receiving intelligence of what is doing on the continent of Europe ſo late as I do here, it is probable that I ſhall be anticipated in my obſervations, I ſhall lay them before you, and, with your approbation, before your readers, after obſerving, that I have lately received the fourth volume of your excellent *Journal*, but am ignorant of all that has been done ſince the publication of it.

Its admirable effects not admitted to prove the decompoſition of water.

I cannot help expreſſing my admiration of the ingenuity with which your correſpondents and others have purſued this moſt curious ſubject; and in general my reſults are the ſame with theirs, though I draw different concluſions from them, eſpecially with reſpect to the modern hypotheſis of *the decompoſition of water*, which, though almoſt univerſally received at preſent, I conſider as wholly chimerical, and unable to ſtand its ground much longer. Indeed, I perceive that doubts are entertained concerning it by ſeveral of your correſpondents, and others obſerve that theſe experiments give no ſupport to it. To me it is evident that they are far from doing ſo. For though it may happen that the inflammable air from the wire connected with the ſilver end of the pile, be in the proportion to the dephlogiſticated air from the wire connected with the zinc end, which that hypotheſis requires, it appears that the latter comes from the air that is merely held in ſolution in the water in which the proceſs is made; ſince if, by means of oil upon the water, or a vacuum, acceſs to the atmoſphere be cut off, the whole production of air ceaſes. There is alſo no production of air when the water has been exhauſted of it; and certainly no good reaſon can be given why, if the water itſelf

For the oxigen does not bear the alledged proportion to the hidrogen; but ariſes only from air diſſolved or abſorbed by the water.

The air ceaſes to iſſue, if the at-

itſelf conſiſts of theſe two kinds of air, and this proceſs be capable of decompoſing it, air ſhould not be produced from it in all theſe caſes; both the conſtituent parts of the water being preſent, and the power of ſeparating them being in full operation. Beſides, I find that though the two kinds of air be produced, they are not always in the proportion required by the new theory, the dephlogiſticated air being much leſs than is requiſite. I have alſo found it not much better than atmoſpherical air. The inflammable air I believe to be of the pureſt kind.

mofphere be excluded by oil or a vacuum.

Other facts.

If this inflammable air come from the decompoſition of the water, the water from which it is extracted ought to contain an overplus of oxigen, either in the form of dephlogiſticated air, or of acid. But the ſigns of acidity bear no proportion to the quantity of inflammable air produced, and can hardly be perceived at all. I did perceive it when I made the proceſs in water tinged with the juice of litmus, but only by the redneſs of the froth from the wire connected with the zinc end of the pile, the liquor itſelf remaining unchanged, notwithſtanding a copious production of inflammable air from the other wire. Alſo, when I introduced a piece of raw fleſh inſtead of the metal connected with the zinc end of the pile, no air came from it, nor did I perceive that the ſurface of it had acquired any acidity, though inflammable air was produced in great plenty from the other wire.

The ſuſpended metal *ſuppoſed to be ſuperſaturated with phlogiſton.*

But, except gold or platina be connected with the zinc end of the pile, there is ſeldom any production of air from that quarter, the metal in that ſituation being diſſolved; and there is no appearance of its being diſſolved by any *acid*, but, on the contrary, of its being ſuperſaturated with phlogiſton. But before I produce the evidence of this, which affords an argument againſt the decompoſition of water that appears to me to be perfectly deciſive, I ſhall relate ſome circumſtances concerning this ſolution of metals, which I do not find to have been noticed by others.

Metallic ſolutions by galvaniſm.

In general, wires connected with the zinc end of the pile are diſſolved, but none ſo readily as thoſe of ſilver, even when the wires connected with the ſilver end of the pile are of the ſame metal, and give air copiouſly; but if zinc or iron be connected with the ſilver end of the pile, any other metal, except gold or platina, connected with the zinc end will be

diſſolved.

dissolved. Iron and zinc were the means of dissolving each other. But the surest method of producing this solution of metal was by connecting *charcoal* with the silver end of the pile. I once dissolved pure gold in this manner, and I preserve the solution as an evidence of it; but I could never do it a second time, though I tried charcoal in several states, perfect and imperfect, &c. nor could I by this process dissolve platina.

Gold.

Charcoal not dissolved, &c.

Charcoal itself is not sensibly dissolved in this process, and air comes from the pieces connected with both ends of the pile. Suspecting that this air might be that which always comes from charcoal when it is plunged in water, I filled the pores of two pieces of it with water, by means of the air pump, leaving them a long time *in vacuo*. Being then tried, they gave no air of some hours, but from the piece that was connected with the zinc end of the pile there proceeded a white cloud, which filled part of the vessel of water. This, however, soon disappeared, the water becoming transparent again; and after some hours both the pieces of charcoal gave air as copiously as any of the metals had done, and continued to do so as long.

Iron; zinc; copper.

When iron was connected with the silver end of the pile, and copper with the zinc end, the latter was dissolved, but not till after two or three hours. Zinc being connected with the silver end, and copper with the zinc end, the former gave air copiously from the beginning, but it was near two hours before the copper began to dissolve, which (being a flat piece) it did at the corners and edges only, and never from any part of the flat surface. When I added more copper, it began to give air without dissolving, and also some of the green precipitate, which had been formed before, gave out air, and, the bubbles adhering to it, it rose from the bottom of the vessel to the top. This precipitate from the copper, which at first was green, became afterwards of a dark brown, as if the metal had been revived. This too was the case with one vessel in which a solution of silver had continued some time. It has given a coating to the glass that is perfectly white and brilliant.

In four vessels of water, with wires forming the circuit, the pheno-

Having introduced four vessels of water between the two ends of the pile, and having connected each two with silver wires, that leg of the wire, in all the vessels, which was next to

to the silver end of the pile gave inflammable air, while, in all of them, the other leg of the same wire was dissolved. When I covered one of these vessels with oil, the production of air and the solution of the wires ceased in them all.

mena ceased, when one of them was covered with oil.

No acid found when silver was dissolved.

Though, as I have observed, there was a slight appearance of acidity in the water when dephlogisticated air was given out at the wire communicating with the zinc end of the pile, there never was the smallest appearance of it when the metal was dissolved. When silver was dissolved in water tinged with the juice of litmus, and there was a copious production of inflammable air from the opposite wire, I could not perceive the least change of colour in the water.

The air in the water was worse than common air.

I examined the water in which the process was made, especially when silver was dissolved in it, but was so far from finding the air contained in it more pure than before, that it was evidently less so. Before the process, the standard of this air, with an equal quantity of nitrous air, was 1.1; with the water made turbid and white with the solution of silver, it was 1.2; and after standing till it became black, it was 1.3.

The black powder of silver contains hidrogen, &c.

The black matter from this solution of silver did not contain any oxigen, but was evidently the metal superfaturated with phlogiston; for when it was heated in dephlogisticated air, it diminished it, and converted part of it into phlogisticated air; and when it was heated in inflammable air, it added to the quantity of it, and this appeared, by its explosion with dephlogisticated air, to be as pure as other inflammable air; so that this black powder of silver is similar to the black powder of mercury made by agitation in water, which I have shewn to be mercury supersaturated with phlogiston. Where, then, is the oxigen that ought to be produced in great quantity, if the inflammable air from the wire connected with the silver end of the pile came from the decomposition of the water?

The glass vessels are blackened, as with hidrogen.

The glass vessels in which silver has been dissolved in these proportions are tinged of a dark colour, which no acid, nor any other menstruum that I have applied, will take out. This is similar to the case of flint glass becoming black by heating inflammable air in it, the calx of lead in the glass uniting with the phlogiston of the air. In this case, therefore, it is natural to infer that this calx of silver imparts phlogiston to the glass, and that there was nothing of oxigen in it. The surface of this black powder of silver long exposed to the air while it is

moist

moist becomes white, which is similar to the experiment with mercury, in which the black powder of this metal, produced by the agitation of it in water, becomes white running mercury as it becomes dry, diminishing and phlogisticating the air in which it is confined.

Experiment to shew that the black powder of silver, treated by nitrous acid, contains no oxigen.

The black calx of silver, made by its solution in nitrous acid, is said by Mr. Macquer, in his Dictionary, to be owing to the phlogiston contained in that acid. To ascertain this, I dissolved some pure silver in spirit of nitre, and evaporating the solution, I heated the residuum in thirteen ounce measures of dephlogisticated air, of the standard of 0.82, with two equal quantities of nitrous air, by which it was reduced to eleven ounce measures, of the standard of 1.72, with one measure of nitrous air; so that it was almost wholly phlogisticated air. Consequently this calx of silver contains no oxigen.

Theory. That in the oxidation of the zinc there are plus and minus states of phlogiston produced in the apparatus, and that phlogiston is electricity, or nearly so.

My present opinion concerning the theory of this curious process is as follows. Since the operation wholly depends on the calcination of the zinc, which suffers a great diminution of weight, while the silver is little affected, and all metals lose their phlogiston in calcination, what remains of the zinc in a metallic form in the pile, and every thing connected with that end of it, is supersaturated with phlogiston, while the calcined part, and every thing connected with that end of the pile, is deprived of it. The former, therefore, is in a *positive* state, and the latter in a *negative* one, with respect to phlogiston; and it seems to follow from these experiments, that this is the same thing with positive and negative electricity; so that the electric fluid and phlogiston are either the same, or have some near relation to each other. The silver seems to act principally as a conductor of electricity; for the surface of it is only blackened in some places in this process, in consequence probably of receiving phlogiston from the zinc; but the water is most essential to it, because it constitutes the principal part, if not the whole, of the addition of weight in the calx. Accordingly, in the calx of zinc I have found nothing but water, though it is probable that there is a small portion of oxigen in it.

or two electric fluids, oxigen and phlogiston.

These experiments favour the hypothesis of *two electric fluids*, the positive containing the principle of oxigen, and the negative that of phlogiston. These united to water seem to constitute the two opposite kinds of air, viz. dephlogisticated and inflammable.

These

These experiments tend likewise to confirm the conjecture which I advanced in my first publication on the subject of air, concerning *the similarity of the electric matter and phlogiston;* and, together with the proper galvanic experiments, shew, that the same substance elaborated from the aliment by the brain is the cause of muscular motion, the nerves being the most sensible of all electrometers. See the first edition of my Experiments on Air, vol. I. p. 274, &c. Muscular motion.

I see no occasion to suppose, with Mr. Volta, that there is any circulation of the electric fluid in this pile. The calcination of the zinc supplies phlogiston as long as it continues, and when that ceases, the operation of the pile ceases with it. I also see no necessity that one end of the pile should be silver and the other zinc; and when both are silver, or both zinc, the operation is the same, nor can I conceive why it should be otherwise. When the pile is properly prepared, the addition of any kind of metal to the ends only serves as a conductor of the electric fluid; and silver, zinc, or any other metal, will sufficiently answer this purpose. No circulation of electricity supposed.

Had this process succeeded without any atmospherical air incumbent upon the water in which it is made, it would have amounted to a full proof of the new theory, one part of the water being deprived of hidrogen, while oxigen abounded in the other, and both of them with the assistance of *caloric*, (though it does not appear whence that could be supplied), assuming the form of air. But this not being the case, the element of the dephlogisticated air evidently coming from the superincumbent atmosphere, the element of the inflammable air must necessarily come from the calcined metals which is a sufficient proof of the doctrine of phlogiston. Whether in this you will agree with me or not, I am, Remark.

Dear Sir,

Yours sincerely,

J. PRIESTLEY.

Northumberland, in Pennsylvania,
Sept. 16, 1801.

P. S. In your *Journal*, vol. IV. p. 226. it is said, " the inventor of the galvanic pile discovered the conducting power of charcoal;" whereas it was one of my first observations in

electricity,

electricity, made in 1766, and published in the first edition of my *History of Electricity* in the year following. See that edition, p. 598.

Experiment of the pile under a receiver, which completely deprived the air of oxigen.

Second P. S. After the above was written, I covered the whole pile with a large receiver, standing in water, charcoal being connected with the silver end of the pile, and silver with the zinc end, in two vessels of water; when the solution of the silver took place in both the vessels, and the air within the receiver began to diminish. The diminution having come to its maximum in about a day and an half, I examined the air within the receiver, and found it completely phlogisticated, being not at all affected by nitrous air. There was evidently, therefore, no dephlogisticated air generated, the whole result being the effect of the calcination of the zinc. This experiment, added to that on the black *calx of silver* produced in this process, and on the *water* in which it is made, completes the proof of there being no decomposition of water in this case, and strengthens the argument in favour of the doctrine of *phlogiston*.

IX.

Experiments and Observations towards determining the Influence of Oxigen on Germination. By Dr. CARRADORI *.

History of the subject. Authors: Boyle, Homberg, Muschenbrock, Boerhaave, Achard, Ingenhousz, Humboldt, Decandolle.

IT was long ago ascertained, by the experiments of Boyle and Homberg, that seeds require air for their germination, for it was proved that they do not germinate *in vacuo*. This necessity of air was afterwards confirmed by the experiments of Muschenbrock and Boerhaave, who had supported the observations of all times by the remark, namely, that seeds buried very deep under ground do not germinate, and that a great number of seeds do not even germinate under water, but perish in that situation. When this subject was afterwards studied with greater attention, in order to ascertain the reason why air is indispensible in this operation, it was found to be necessary in consequence of the portion of vital, or oxigen air which it contains: For Achard in the first place, and after

* Journal de Physique, LIII. Vendemiaire. 10.

him other philofophers, have fhewn that germination does not take place in any mephitic air; as, for example, inflammable or hidrogen air, phlogiftic or azotic air, which are known to contain no oxigen; whence it was neceffarily concluded, by a legitimate inference, that of all the atmofphere which we term the air, the oxigen is the only portion neceffary to germination. This has been confirmed in a more direct manner by Ingenhoufz, who difcovered, that the greater the abundance of oxigen the greater is the rapidity and facility of germination in grains or feeds; which probably led Humboldt to the difcovery of the means of accelerating germination, and effecting it even in the moft obftinate feeds, by the oxiginated muriatic acid, or even, as Decandolle pretends, of the nitric acid, which are known to be charged with oxigen.

At prefent it remains to be afcertained what influence oxigen exerts upon growing feeds; that is to fay, what precifely are its effects upon germination. In order that germination may take place, it is neceffary that there fhould be a principle of fermentation in the farinaceous fubftance which refides in the cotyledons or placentæ of the feeds, which fubftance, as every one knows, envelopes and furrounds the germen or embryo; hence there is reafon to believe that this fubftance, combining with oxigen by means of fermentation, acquires a degree of acidity, and confequently a ftimulus proper for exciting the vitality of the germen, and for giving the firft impulfe to the circulation of the fluids in the embryo; or that, by means of the oxigen, this fubftance becomes modified fo as to acquire particular properties, by which it is rendered capable of affording the firft nourifhment to the tender fœtus or embryo of the plants; fince it has been demonftrated, by the late experiments of Dr. Rollo, that in the germination of the farinaceous feeds, the amylaceous part is converted, by means of the concurrence and combination of the oxigen, into faccharine fubftance, or even both. **Oxigen is thought to produce a change in the amylaceous part of feeds.**

It is not certain, however, that oxigen influences this only; that is to fay, that thefe are its fole effects: in order therefore to afcertain this point, I made the following experiments. **Experiments.**

I placed feeds or grains of wheat, *triticum*, in a plate with water, in the following manner: I fixed them to the bottom of the plate with foft wax, fo as to make them remain in a perpendicular or ftraight direction, fome with the germen upwards, **The feeds of wheat germinate moft fpeedily when the germ is in contact with oxigen.**

wards, others with the germen downwards, and fo that the water reached precifely to the level of their height; that is to fay, that it did not entirely cover them, in order that one of their extremities might be in contact with the air. The feafon was favourable to germination, it being the month of September, when the thermometer indicated 17° Reaum. or about 21° of the thermometer of 100 degrees. On examining them twenty-four hours afterwards, I found that thofe which had the germen upwards, or in contact with the air, germinated remarkably well, though all the reft of the fubftance of the feed was immerfed in the water; and that the others, which had been placed in the water with the germen downwards, though one of their extremities was equally in contact with the air, namely, the extremity oppofite to the germen, could not well germinate; they merely exhibited fome figns of germination; and there was no difference to be perceived between thofe grains and others which I had placed on their fide, not upright or perpendicular to the bottom of the plate, but lying down horizontally, and confequently covered entirely with water. Thus it appears that perfect germination does not take place unlefs when the part which contains the germen is in contact with the air, although all the reft of the feed be guarded againft its immediate contact.

Sprouted wheat grew well where the developed plant touched the oxigen; but did not when it was covered.

I took feeds or grains of wheat which had already germinated in the open air, and by means of the fame wax I placed them in the water, fome with the germen upwards, others with the germen downwards, in the fame veffel, and with the fame precautions, namely, not fuffering the water entirely to cover them; fo that thofe which had the developed germen, or the fmall plant directed upwards, were in contact with the air at this point only. When I examined them again, I found that thofe which had the germen downwards, and which confequently were not in contact with the air at this part, remained as they were, and exhibited no other fign of further germination; whilft in the others, whofe germen was expofed to the air, the germination advanced, and the fmall plant increafed remarkably in growth.

The ftalk is the part where the oxigen ought to apply.

I afterwards wifhed to afcertain, if it were indifferent whether all parts of the germen were in contact with the air, or only fome of its parts. For this purpofe, I chofe a number of grains of wheat which had germinated well for twenty-four hours

hours preceding, ſome of which I placed in the water with only the ſtalk projecting out of it, whilſt all the remaining part was immerſed under its ſurface, and others with the ſtalk under water, and the ſmall roots above its ſurface. I did not find that the germination made any progreſs with thoſe which had their ſtalk under water; but thoſe whoſe ſtalks were in contact with the air thrived and advanced in growth, and in a few days time the plant was developed.

Thus oxigen, or vital air, is neceſſary to germination, ſince the tender plant cannot thrive and grow without its immediate contact. Thus we ſee, that though it has received life by the effect of germination, it cannot preſerve it unleſs it be in contact with the air at this part; that is to ſay, at the ſtalk, which poſſeſſes an organiſation adapted to receive its benign influence, which is neceſſary to the ſupport of life. The germination commences without the immediate contact of oxigen, but it cannot become vegetation, if I may ſo expreſs myſelf, without its immediate contact. It ſeems, that in order to animate the germen or embryo of plants, and give it life, by means of the circulation and other functions, combined oxigen is ſufficient, but that it cannot afterwards ſubſiſt, if this element in a free and pure ſtate is wanting to the tender plant.

Germination may begin without free oxigen; but vegetation cannot proceed without it.

In fact, as I have already remarked, and as I have obſerved ſtill better in the ſubſequent experiments, the ſeeds begin to germinate under the water, but their germination ceaſes and proceeds no farther, unleſs the air receive the plant already born, or the animated germen. But germination is not effected under water unleſs in a certain caſe.

I ſelected a number of good ſeeds or grains of wheat; ſome of which I put into a plate, and poured in as much water as was preciſely neceſſary to cover them; others I put into a deeper veſſel, and covered them with a larger quantity of water; others I put into a glaſs, and covered them with a large quantity of water; and, laſtly, I put others into a bottle with a narrow neck, and covered them ſo that the water reached to the neck of the veſſel. After twenty-four hours, I found that the ſeeds in the firſt and ſecond veſſel had begun to germinate, but none of thoſe in either of the two laſt veſſels germinated, nor did they afterwards.

But no germination takes place unleſs the water be expoſed to the air, &c.

 Hence

Hence it clearly appears, that as this difference can be referred only to the diverfity of the veffels, that is to fay, to the different furface which the veffels in which the feeds had been placed expofe to the air, and to the different quantities of water in which they had been immerfed; or, in other words, to the difference of the action or influence exerted by the oxigen contained in the atmofphere upon the water: germination cannot even commence without the immediate contact of vital or oxigen air; provided, however, that the water in which the feeds are immerfed be in a condition to become changed with the oxigen, which is neceffary to effectuate fuch a function. It cannot be doubted but that water powerfully attracts a large quantity of oxigen from the air; it has a ftrong avidity for this principle, and becomes abundantly loaded with it. M. de Gleichen and Senebier had obferved that the feeds of peafe, when placed in too large a quantity of water, perifhed without germinating, but they had given no accurate explanation of the fact.

under circumftances favourable to its abforption of oxigen.

Recapitulation.

I have thus eftablifhed two effential facts relative to germination, which, to the beft of my knowledge, had not before been pointed out. Vital or oxigen air is neceffary to the grand procefs of germination; but in order to give the impulfe or the principle of this germination the immediate contact of the air is not neceffary, but it is indifpenfable to its continuation or progrefs; fince the germen already animated, or the fmall plant, cannot grow nor vegetate, unlefs it be in a ftate to enjoy the immediate influence of this vital fluid. Ingenhoufz had only afferted in general terms, that the contact of oxigen is neceffary to the germination of grains or feeds; but fomething more particular was ftill wanting.

Repetition of experiments, with other plants.

I have repeated the fame experiments upon the feeds of barley, *hordeum vulgare*; of beans, *vicia faba*; lupines, *lupinus albus*; and have obtained the fame refults: hence we may conclude that the above-mentioned inferences are applicable to the feeds of all *terreftrial plants*, that is to fay, fuch as fow themfelves upon the dry ground.

Seeds of aquatic plants germinate and grow by the combined oxigen of water.

As to the feeds of aquatic or marfh plants, it appears, that, both for the commencement and for the continuation of germination, the oxigen which is combined with the water is fufficient, without the concurrence of free oxigen, as we fee that they germinate, and afterwards grow very well, under the

the water. Their organifation, different from that of the terreftrial plants, enables them to avail themfelves, for the fame operations, of the oxigen which is diffolved in the water.

Seeds are injured by lying in water.

Though feeds can germinate under water, yet by being kept there they lofe much of this faculty, and at length become altered to fuch a degree as to be no longer capable of germination, even though we change their place, and tranfport them into dry earth.

Experimental proof,

I have obferved that feeds of wheat, that had been kept in water during three fucceffive days, but not under circumftances to enable them to germinate, and were afterwards tranfported into dry ground proper for producing this effect, germinated almoft with the fame vigour as if they had been placed there at firft; but the fifth day the greater part ceafed to germinate, and thofe which ftill germinated did it in a languid manner, and the fmall plants which they produced were fickly, and without vigour.

even after germination.

I have remarked, that the fame happens with plants which have germinated under water; by remaining there during too great a length of time, thefe tender plants are fpoiled, and no longer capable of growing and thriving, even though they change their fituation, and are placed under more favourable circumftances. I have alfo obferved, that this too long abode in the water is the more dangerous to them when thefe plants are tender; that is to fay, nearer to the period of their birth, or commencement of germination.

Hence the mifchief of abundant rains after feed time.

Hence we fee what danger arifes from too long continued and abundant rains after the feed time. The too great quantity of water keeping the feeds for too great a length of time in a ftate of fubmerfion, prevents their germinating, or injures their healthy germination, and is the caufe why the plants which they produce appear with a conftitution more or lefs altered, in confequence of the prevention of their communication with the air, or, in other words, for want of oxigen. Hence the wet winters, which, for the reafons that have been mentioned, may prove dangerous to the feeds of wheat, the moft important of the fruits of the earth, are juftly confidered as prefaging a fcanty harveft.

> ———Hyemes optate ferenas
> Agricolæ: hyberno lætiffima pulvere farra
> Lætus ager——— VIRG. GEORG.

Dry windy winters are best for the seed.

Dry windy weather is then advantageous, since the oxigen necessary for the grand work of the germination and growth of the tender plants is furnished easily and abundantly by the atmosphere. After this period, that is to say, after their infancy, the plants no longer require oxigen, in order to prosper, but a mephitic air, as I have already shewn in my memoir *On the Fertility of the Earth*,* because in this they find their principal nourishment: thus when the period has elapsed, oxigen is but of very limited utility to vegetation.

X.

On the Choice and Use of a Razor. By a Correspondent.

To Mr. NICHOLSON.

SIR,

January 26, 1802.

If you think the following observations applicable to your paper on Shaving, you will insert them in some future Number of your Journal.

W. B.

On the choice of a razor.

In choosing a razor (besides the marks you enumerate) prefer that blade whose edge is least blunted or turned after being two or three times drawn upon its edge, from heel to point, on a bit of horn or the thumb nail.

Straping.

In straping care should be taken to give the finishing strokes in the direction in which it is about to be used: that is, if the drawing stroke in shaving is made from heel to point, the razor should be drawn from heel to point upon the strap, and *vice versâ*. This observation applies to all fine edges, especially to surgical instruments. For a strong beard, a rounder edge is given by a loose strap; but a keener and more durable one is given by a strap fixed upon some inelastic substance.

* Della Fertilita della Terra. Memoria del Doct. J. Carradori, Premiata della R. Societa Economica di Firenze nel mese di Giugno del 1799.

The

The lather.

The foap lather, befides the effects already mentioned, feems to act by giving a degree of firmnefs to the beard. It unites, in fome meafure, a great number of hairs, and thereby occafions a more equal refiftance to the ftroke of the razor, diminifhing the elafticity of the hair; and alfo taking off a little of its natural fine polifh. In fhaving without foap the beard is generally irregularly cut, efpecially if the edge be not perfectly keen; as may be obferved in cutting a bundle of briftles loofely tied, or taking the edge off a quire of paper without putting it into a prefs. Provided the lather be of fufficient confiftence, it is not material whether hot or cold; for I fufpect the alkali acts pretty quickly upon the polifhed furface of the hair.

The form of the blade.

The form of the razor is of fome confequence. Young fhavers, who may not be equal to giving the drawing ftroke, fhould choofe a fcimitar-like blade. Every razor ought to have the point terminated by a fegment of the circle; otherwife it is difficult to give the ftroke from the point towards the heel, which is often neceffary when a man is not *ambo dexter*.

Effect of heat on edge tools.

Another Correfpondent has favoured me with what feems to be the moft probable explanation of the effect of heat on edge tools. He obferves, that in the cold regions of North America an axe will fometimes fly in pieces like glafs, and that our fmiths in this country are well aware of the increafe of tenacity which a very flight increafe of temperature gives to fteel, iron, and other metals, and take care to ufe it when they fet or alter the figure of any tool or utenfil by cold hammering. They warm the article to prevent its breaking. He thinks the fine edge of a razor, which would fplinter and become rough if ftrapped or ufed cold, may perform its office much better when rendered more tenacious by a moderate heat.

W. N.

XI.—*Ob-*

XI.

Obſervations on the Method of painting with Milk. By CITIZEN DARCET, *Member of the Lyceum of Arts, and Eſſayer of Money.* *

Importance of ſubſtituting caſeous matter for ſize in painting.

UPON reading the different articles of the Decade Philoſophique, in which the method of painting in milk is mentioned, and on examining the theory which Cadet-de-Vaux has developed reſpecting this uſeful application of our chemical knowledge, I perceived how important it would be if we could ſubſtitute the caſeous or cheeſy matter, which we poſſeſs in ſuch great quantities, to glue or ſize, which is commonly uſed in painting in diſtemper, and by this means to appropriate that ſubſtance to paper-hangings and other arts, for which it is an article of the firſt neceſſity. I apprehended that the proceſs of this new method of painting was ſuſceptible of being rendered more ſimple, and I endeavoured to ſtudy the different phenomena in order to aſcertain what ſubſtance might be left out in part, or even wholly, without altering the goodneſs of the colour. This examination has enabled me to make the following obſervations, which may ſerve as a ſupplement to the memoir publiſhed on that ſubject. †

Hiſtorical remarks.

I ſhall not attempt to ſolve the queſtion, whether painting in milk was known to the ancients. The ſolution of this problem, which is perhaps impracticable, is ſcarcely intereſting to us. I ſhall only remark, that the Indians, who uſed milk to dilute the colours with which they painted the ſides of their cabins, appear to have given the firſt notion of the application of a natural mixture of the caſeous part along with the ſerous part, to render colouring matter adheſive. ‡

For this application we are indebted to Cadet-de-Vaux, who has by theſe means afforded a real ſervice to the public, by

* Decade Philoſ. No. 5, An. X.

† See Philoſ. Journal, quarto, V. page 217.

‡ Our houſe painters are alſo acquainted with the advantageous uſes of milk in inſide paintings. They have long uſed a mixture of milk and well waſhed lime to give a brighter white to delicate objects in relief, in order that they might appear more prominent, from the ceiling painted with the ordinary white-waſh.---D.

by rendering a material ufeful which in many places had, properly fpeaking, little value, unlefs in the ordinary ufe of extracting the butyraceous part, which alone reprefented the whole value of the milk before its decompofition. He has at the fame time given a degree of perfection to this paint, by rendering the procefs fufficiently fimple, to afford a folid colour, nearly without fmell and at a moderate price.

Improved procefs for painting.

Thefe qualities, which painting in milk really poffeffes, appear to render it little fufceptible of amelioration. I fhall neverthelefs venture to propofe one which has fucceeded in my hands, and of which the experiments have been made on a fcale fufficiently extenfive to infure the goodnefs and the value of the refults I have obtained. *

After having given an account of the obfervations which led me to this procefs, I fhall fpeak of the materials I ufed, their proportional quantities, the beft method of combining them, and, in a word, the procefs to which we muft juftly apply the denomination of cheefe painting. †

Cadet-de-Vaux's two proceffes do not differ. Critique on his method.

Citizen Cadet-de-Vaux, in his memoir on milk painting, has given two proceffes, which, as it appears to me, ought to be confidered as one, becaufe the firft which he announces as fimilar to common diftemper does not differ from the fecond, which he gives as proper to be fubftituted inftead of oil painting, except in the Burgundy pitch, which conftitutes part of this laft. In fact, Burgundy pitch renders the colour more folid, but it is far from rendering it capable of being wafhed like oil paint; a property which is known to be truly charafteriftic of this paint, and is of itfelf fufficient to diftinguifh it from diftemper. I fhall not, therefore, examine thefe two proceffes feparately; and I fhall conclude my ob-

* I painted an entire chamber with the colour I fhall proceed to defcribe. Individuals, who flept in it the very day of the operation, were in no refpect incommoded by the fmell of the paint, though the door and the windows were fhut the whole night.

† We find, in the *Dictionnaire de Peinture de Pernety*, a note in which he fpeaks of a pamphlet entitled, *La Peinture au Fromage ou au Ramekin*. It was written in oppofition to the encauftic method of painting, of which Citizen Bachelier has, as it were, re-invented the procefs. I have not been able to procure this pamphlet, for which I am very forry, as it might probably contain fome data refpecting the method of painting on which we at prefent treat in a more ferious manner.---D.

ject to be attained, if the procefs I offer fhall afford a colour equally good as that which is obtained by following the procefs for the refinous milk paint.

We read at page 250 of the memoir to which I allude, that the oil added to the mixture of flaked lime and fkimmed milk is diffolved by the lime, and then forms a calcareous foap.

Lime, milk, and oil form a triple compound.

By carefully examining what paffes in this operation, I have thought I obferved that the lime does not feparate from the cafeous part to combine with the oil; but that the oil which is added in the mixture forms a triple combination, little foluble in fact, but perfectly diffufible in water. We know, on the contrary, that calcareous foap is infoluble and quite immifcible in water; and I have found that the addition of fkimmed milk produces no change in thefe properties. I have alfo remarked, that the triple combination does not take place, but in the order defcribed by Cadet-de-Vaux, for the lime does not entirely combine with a mixture of oil and fkimmed milk. In this cafe there is only a formation of calcareous foap, which remains in maffes fufpended at the furface of the liquid.

Sour milk unfit for the pigment.

Cadet-de-Vaux alfo announces, in page 248 of his memoir, that four milk is no longer proper for the compofition of this colour. He obferves, that the ferous part of the milk being converted by fermentation to the ftate of acetous acid, may then form a kind of calcareous acetite, which, by its deliquefcence, muft contribute to deftroy the colour in which it is mixed.

The whey is of little utility in the paint; but very ufeful in many other applications.

I have verified this fact, and I think that the ferous part cannot be ufeful in the compofition of paint, unlefs before that period in which the acefcence fhall have converted the fugar of milk it contains into acetous acid. For in the former cafe it is capable of giving folidity and brightnefs. But when we confider that the fermentation which produces this phenomenon takes place with much rapidity; that in many countries this ferous part is ufed in bleaching, in the preparation of fugar of milk, and for the feeding of animals, we fhall be defirous of retrenching it from a procefs in which it is fometimes ufeful, but oftener noxious, in order to appropriate it entirely to thefe feveral ufes.

By

By examining, in the fame manner, the effect of the addition of oil upon the reft of the mixture, we foon perceive that if it does not improve the folidity of the colour, it muft either be ufelefs or pernicious; becaufe it renders the tint dull, and communicates a difagreeable fmell. Now experiment demonftrates the negative, for milk paint in diftemper does not refift water more than the fimple mixture of fkimmed milk with flaked lime and whiting.

The oil is of no value as an ingredient.

I have alfo concluded that the quantity of fix ounces, or 183,430 grains of flaked lime, prefcribed in the recipe for painting in diftemper with milk, was much too confiderable, becaufe two ounces, or 61,143 grammes, or even a fmaller quantity, is fufficient entirely to liquify one French pint, or or 951,206 cubic centimeters of fkimmed milk. The pellicle of calcareous carbonate, which is formed after a time at the furface of painting in milk diftemper, fhews, as well as other experiments on this fubject, that the prefent obfervations are well founded.

The proportion of lime is too great.

With regard to the whiting, I think the dofe may be increafed without inconvenience. Cadet-de-Vaux already added three pounds, or 1467,438 grammes, to five pounds, or 2445,75 grammes. I have gone further, and have ufed a dofe of eight pounds, or 3913,168 grammes, and the colour I obtained did not appear to me lefs folid than that made according to the original recipe. This obfervation does not apply to the preparation, when required to be coloured either by an ocre, an earth, or an oxide. I have remarked, that thefe fubftances require more of the mordant than the whiting does; and this happens, in fact, with painting in diftemper with milk coloured yellow or red, in order to be applied to pannels, &c. namely, that the addition of thefe colouring matters deprive it of all its folidity. We muft not, therefore, add the colour to the paint entirely mixed, as is directed at page 248 of the memoir, but diminifh the dofe of whiting in proportion to the quantity of colouring matter neceffary to produce the required tint; in a word, it is requifite that the colouring matters added to the whiting fhould not weigh at moft more than the five pounds, or 244,573 grammes prefcribed in the recipe.

The whiting may be increafed.

Colour muft be added during the firft mixture.

Thefe are the principal obfervations I have made on painting in milk; let us now fee what are the confequence we may juftly derive from them.

I have

An improved composition.

I have concluded that the oil or Burgundy pitch, the ferous part, and a portion of flaked lime, might be entirely left out of the recipe. My paint will, therefore, contain only the caseous part, a portion of flaked lime and whiting. Reasoning pointed out these conclusions; but my doubts on the subject were not yet cleared up by experiment.

I formed a great number of mixtures by constantly varying in each of them, in opposite directions, the doses of cheese, of lime and of whiting. Many of my experiments were useless; but I succeeded in discovering the following process, which appears to me to answer the intended purpose. The following are the doses:

Proportion of ingredients.

Cheese or curd well drained	144 grammes =	5 oz. avoir.
Slaked lime - -	7 grammes =	$\frac{1}{4}$ oz.
Whiting - - -	280 grammes =	10 oz.
Fine powdered charcoal	2 grammes =	1 dram.
Water - - -	80 grammes =	3 oz.

The kind of cheese.

This cheese is commonly called *fromage à la pie*, or soft cheese. I have used old cheese which was almost dry, which nevertheless afforded good results, but the fresh cheese is certainly to be preferred. I must also observe that these cheeses differ from each other; they are not all equally proper for the purpose, and I have found some which compose colours of little solidity; they were, in general, disagreeable to the taste.

Manipulation.

Let us now speak of the manipulation. At the moment of commencing the operation, a certain quantity of strong quick lime must be slaked in the least possible quantity of water. This is the surest and most speedy method of reducing it into fine powder. The lime is to be sifted, in order to separate the pieces which do not fall down, and of the powder seven grammes are to be weighed. The quantity of cheese above indicated is to be taken and pounded till it has the appearance of salve, and with this the seven grammes of lime are to be mixed, and the mixture well agitated, which loses its consistence, and acquires that of hot new made glue.

On the other hand, whiting in powder is taken, and added to the water and the charcoal, and the whole accurately mixed. This mixture may be passed through an open sieve, in order that it may be reduced to a liquid homogeneous paste.

The

The mixture of lime and cheefe is then to be added, and carefully mixed with that of the whiting and charcoal diffufed in water. The colour is then finifhed.

Dilution. The dofes here pointed afford a colour too thick to be ufed in this ftate. We muft therefore add to the mixture a quantity of water neceffary to communicate the degree of fluidity defired; but this addition muft not be made till the moment before it is ufed, for I have obferved that the colour keeps better the lefs water it contains.

Surface covered. Two hundred and ten grammes of water added to the colour, made as before prefcribed, afford the neceffary quantity for exactly covering a fquare furface of 1,948 meters, or one toife or fathom. It may, however, be eafily apprehended that the dofes of water, and even thofe of charcoal, may be varied to a certain point, according to the judgment of the operator.

Colour. When a red or yellow colour is defired, fimilar to that which is ufed for pannels, ceilings, &c. I fubftitute, inftead of whiting and charcoal in the foregoing procefs, the colouring matter which I intend to ufe. The following are the preparations which appeared to me to afford the moft folid colour moft capable of fupporting the encauftic and wax *(l'encauftique et le cirage.)*

Well dried curd or cheefe	-	144 grammes
Slaked lime	- - -	7 grammes
Colouring matter	- - -	200 grammes

Shining effect by wax or fugar. If, inftead of the ocres, we fubftitute charcoal in a ftate of high divifion, lamp black, for example, we obtained a black colour, which may be ufed with fuccefs to blacken the leather of trunks.

When the colour is dry, if it be defired to give it a fhining appearance, it is covered with two coats of a folution of white wax in the effential oil of turpentine. When this kind of encauftic is dry, the wax may be polifhed by friction with a clean cloth. This preparation has the advantage that it does not fcale, but refifts water a little, properties which the compofitions ufually applied to this purpofe do not poffefs.

If, to the mixture of cheefe, lime and lamp black, a fmall quantity of dried fugar or honey be added, a black colour is obtained, which dries fpeedily, and is fufficiently fhining to

be used like the foregoing. The experiments I have made on this object, though satisfactory, are not yet perfectly equal to my wishes.

The paint is mixed according to the preceding instructions, and the necessary degree of fluidity for painting a first or second coat is given by means of water, the dose of which must be determined by circumstances and judgment. The whole is then covered with the common encaustic, and the operation is finished as usual by rubbing.

Advantages of this new paint.

I shall not enlarge more amply on the utility of the paint which I here propose. Every observation which has been made by Cadet-de-Vaux may be applied to this, because the various changes I have made in this process do not deprive it of any of its advantages; but, on the contrary, render it more valuable. The solidity of the painting by means of cheese is at least equal to that of painting with milk and resin; but the former is more beautiful, less disposed to become yellow, is more simple, and must necessarily cost much less than the paint in distemper with milk, and still much less than the resinous paint with milk. Water does not spot it, nor even leave any trace after it is dried, an advantage which size painting is very far from possessing.

It may be kept very well, or made into cakes.

This paint with cheese may be kept very well, particularly when it does not contain much water. I have several times formed it into cakes, which, when pounded after their entire desiccation, formed, by the addition of a very small quantity of cheese and lime, a colour as solid as that which had been newly made. This experiment shews how much the export of this colour manufactured in agricultural districts may become advantageous in the way of trade.

How far this or any other paint can be a preservative against infection.

With regard to the property which the resinous paint with milk possesses of depriving walls of infection by its application upon surfaces penetrated with putrid exhalations; as this effect is purely mechanical, we may by analogy infer that painting with cheese must answer the same purpose, because it is sufficiently adhesive and fluid completely to stop the pores of the stones, plaister, and wood, to which it adheres no less strongly than the resinous paint with milk. But this property appears to me of little real utility. The processes which Citizen Guiton has given in his work on disinfecting the

the air, may be applied to the cafe mentioned by Cadet-de-Vaux, and anfwer the purpofe with infinitely more fpeed and perfection. It appears, therefore, that thofe methods only ought to be ufed in fuch cafes. For the fumigations operate of themfelves, and without the affiftance of a workman; whereas the painter of an apartment or infected chamber muft neceffarily expofe his health, and fometimes even his life.

Suggeftion that it may be ufed in block printing of paper hangings.

It only remains, after having thus endeavoured to fimplify the procefs which Cadet-de-Vaux firft brought to perfection, that I fhould exprefs my wifh that the advantages it really poffeffes might engage the manufacturers of paper hangings to adopt its ufe. The inhabitants of the country, who muft naturally be more ftrongly impreffed with its advantages than thofe of towns, will, no doubt, be the firft to avail themfelves of it. This application may at leaft affift us in faving an annual tribute, which our manufacturers pay to foreigners for animal glue or fize. If cheefe could hereafter be ufed as a fubftitute in block printing, I fhould think myfelf happy to have contributed one ftep towards the perfection of an art no lefs ufeful than agreeable.

XII.

Defcription of an Hydraulic Bellows for a Smith's Forge. By Mr. J. C. Hornblower.

To Mr. NICHOLSON.

SIR,

Small hydraulic bellows for a forge.

I SEND you a fketch of a fmith's bellows a little out of the common way. Its application to my purpofe was fuggefted by an acquaintance I had by the large machines of that fort erected at Willy Furnace, by the late Mr. Ifaac Wilkinfon, father to the prefent Mr. John Wilkinfon, about forty years fince, and where circumftances or inclination have taken the lead, I have adopted the principle occafionally in this form.

Its effect compared with common leathered bellows.

What I have principally in view is, to mention to you a very ftriking difference between the effect of this bellows, and a common leathered 30 inch bellows in the fame fhop. The leathered bellows throws confiderably more air to the fire, and

its nozzle compared with this, is as ,73 to ,60 in diameter, but it does not produce ſo great an effect in bringing on the heat, and the voice of this is ſo great as almoſt to drown that of the common one. The only difference in other reſpects is, that in the hydraulic bellows the pipe goes underground for about eight feet, and the conducting pipe of the other comes down about the ſame diſtance from the ſhop above.

If any further particulars are neceſſary for your comment on this ſubject, they are at your ſervice.

I am, SIR,

Your very obedient Servant,

J. C. HORNBLOWER.

Eaſt Row, City Road, Feb. 12, 1802.

DESCRIPTION. Plate XII.

Deſcription of its ſtructure and operation.

A. The plunger, or working part of the bellows, 18 inches ſquare within, which receives the air by a valve in the hinder part opening inwards, which at the ſtroke by the rockſtaff E, throws it down the tube indicated by the dotted lines, which has a valve opening into the reſervoir D, whence it is led to the tuyere by the pipe P. Length of the plunger 20 inches, ſtroke nine inches. Diameter of P three inches; of the nozzle 0,6 inches.

The whole is placed in a pit or ciſtern, having water ſufficient to riſe to the lower end of the tube where the valve hangs; this tube is the only communication between the upper part and the reſervoir D: when as much water is poured in round the working part over the waſh boards, as will riſe within five inches of the upper edge of them, the bellows is ready for uſe. The little frame work ſerves to keep it from riſing, and affords a convenient ſupport for the balance and the rock ſtaff.

I ſhould obſerve, that the area of the pit or ciſtern ought to be at leaſt twice as much as that of the plunger A *.

* From the preſſure of other buſineſs, it has not been in my power to viſit this manufactory, in order to aſcertain the cauſe of the difference here ſtated; but I ſhall give the facts more particularly in the next number. W. N.

XIII. *On*

XIII.

On the Practicability and Advantages of a general Syſtem of Rail Roads, and the Means of carrying the ſame into Effect. In a Letter from RICHARD LOVELL EDGEWORTH, *Eſq.*

To Mr. NICHOLSON.

SIR,

MANY years ago I formed the project of laying iron rail-ways for baggage waggons on the great roads of England; but having conſulted ſeveral of my friends, who were eminent mechanics, ſo many objections were ſtarted, that I for ſome time deſpaired of ſucceſs. One great objection aroſe from the vaſt expence of maſſive rail-ways, and the continual coſt of repairs. To obviate this difficulty, it occurred to me to divide the weight that is uſually carried upon a ſingle waggon into four or five portions, and to place them upon four or five ſmall carriages; theſe carriages linked together would be as eaſily drawn as the ſame load upon one waggon. In purſuance of this idea, about the year 1768 I preſented models of three ſuch carriages to the Society for the Encouragement of Arts and Manufactures, who for this and other inventions in mechanics, honoured me with their gold medal; the date of which, and the journal of the Society may aſcertain the early claim which I have to this invention. In 1788 I conſtructed four carriages with caſt iron wheels, truly turned and ſupported upon friction rollers; theſe were ſhewn to ſeveral eminent perſons, and were employed upon a temporary moveable wooden rail-way for a conſiderable time, in carrying limeſtone for the improvement of land. A variety of accurate experiments, and ſome uſeful improvements in this mode of carriage were made with theſe machines, which it would take up too much of your valuable work to detail. I ſhall mention only one idea, which appears to me ſo practicable, that I beg a place for it in your Journal, which will ſecure to it the moſt extenſive circulation here, and on the Continent.

The public advantage of rail roads long ago ſtated by the author,

with models, &c. to the Society of Arts; viz. in 1768.

Experiment in 1788.

I propoſe, that by way of experiment iron rail-ways ſhould be laid on one of the great roads, to the diſtance of ten or twelve miles from the metropolis, upon ſomething like the following

Plan for the commencement of a public railway. Four roads; two for

heavy carriages, out and home, and two for light.

lowing plan: four rail-ways ſhould be laid on the road, raiſed on ſleepers of ſtone, ſo that their upper ſurface ſhould ſtand about four inches above the road. They ſhould be made hollow from the bottom upwards, for ſtrength and to ſave expence broad at bottom, and rounded at the top, to prevent the lodgment of dirt and duſt. On theſe ſhould run light waggons, each containing not more than one tun and a half weight.

I have mentioned four rail-ways. The two inſide roads ſhould be appropriated for waggons, and the two external rail-ways for coaches and chaiſes, &c. The left hand rail-way invariably to be followed by each ſpecies of carriage on its own road; ſo as to prevent the poſſibility of any carriages meeting on the ſame rail-way. By appropriating the exterior tracks to light carriages, thoſe which wiſhed to paſs others might turn off upon the waggon road, and reſume their proper place after they had gone by the carriage they wiſhed to paſs. Now to accommodate coaches and chaiſes, &c. to theſe rail-ways, I would have them carried, wheels and all, in cradles or platforms, *which ſhould have wheels adapted to the rail-ways.* By theſe means no alteration would be neceſſary in any of the carriages commonly uſed; but the horſes of any coach or chaiſe might, as ſoon as they had got out of town, walk up an inclined plane into the cradle or platform, and draw their reſpective carriages after them: the horſes ſhould then walk out at the fartheſt end of the platform, upon the road belonging to the rail-ways. They would then draw the chaiſe not upon its own wheels, but upon the wheels of the platform or cradle in which the *chaiſe* ſhould be detained.

The carriages to be drawn ſtanding on platforms or cradles adapted to the rail.

Expected rate of travelling, 12 paſſengers, ſix miles an hour by one horſe, &c.

For ſtage-coaches ſimilar platforms ſhould be provided, and in theſe ſix inſide and ſix outſide paſſengers might travel at the rate of ſix miles an hour with one horſe. Hackney, or gentlemen's chaiſes might go at the rate of eight miles an hour with one horſe, without interruption or delay.

Hills avoided.

Where hills intervene, new roads muſt be made following the courſe of ſtreams that wind between the hills, a moderate acclivity would not obſtruct the progreſs of theſe carriages, that is to ſay, a riſe of one foot in ten.

Numerous benefits from this ſcheme.

Every perſon converſant in theſe ſubjects, muſt know how much within bounds I ſpeak with reſpect to the eaſe of draught upon rail-ways. The ſaving of horſes and their food, the ſaving of wear and tear of carriages, the increaſed diſtance to which

which horfes could travel in a day, the freedom from dirt and duft, the fecurity by night, the eafe with which the fick and infirm might be tranfported from place to place, are all obvious confiderations; but the chief convenience of this project arifes from the mode of receiving and tranfporting on rail-ways every carriage now in ufe *without any change in their ftructure*, fo that a traveller may quit and refume the common road at pleafure. To enumerate all the advantages that would arife from fuch roads, is unneceffary in this flight fketch; the intelligent reader will probably perceive many that are not even hinted. It is felf-fufficient to lay the general idea before the public; the contrivance and economy of the different parts of fuch a project require long and minute details, which muft be referved for another ftage of the bufinefs. If fuch a plan fhould have a fair trial, it might lead the way to farther fpeculations.

In this plan the carriages require no alteration.

It is not impoffible by *flight circulating chains*, like thofe of a jack running upon rollers, to communicate motion between fmall fteam-engines, placed at a confiderable diftance from each other; to thefe chains carriages might be connected at will, and when neceffary they might inftantaneoufly be detached. What a prodigious faving of expence might be thus effected? If the freedom and facility of intercourfe, which has been obtained by good roads and canals, be, as Adam Smith afferts, one of the grear caufes of our national wealth, how far might this freedom and facility of intercourfe be extended by the perfection of the fcheme, whofe outlines I thus lay before the public.

Draught by fteam engines.

Every great project requires time for confideration; time accuftoms the public mind to new views, and what at firft appears too diftant and unattainable by *time* becomes familiar and practicable.

The detail and reception of every great project requires time, &c.

Mechanics will not fail to comment on what appears in fuch a refpectable publication as yours. You will not, Mr. Editor, hefitate to give your own opinion; you will at leaft be certain, that whatever objections are raifed will be treated with candour, and replied to without enthufiafm, by

Your humble Servant,

RICH. LOVELL EDGEWORTH.

Edgeworth Town, Ireland.

XIV. *Defcription*

XIV.

Description of the darkening Apparatus for Telescopes by Means of Fluids. By WILLIAM HERSCHEL, *L. L. D. F. R. S.* * *with an Engraving. Plate XI.*

Construction of an apparatus for viewing the sun through a coloured fluid.

A B, Fig. 1. is a square trough, closed at the two opposite ends C D, by well polished plain glasses. It will hold any liquid through which the sun's rays are to be transmitted. E is a small spout, and F a handle; so that any portion of the liquid may be conveniently poured out, when the rest is to be diluted.

The trough is made to fit into the open part of the skeleton eye tube, Fig. 2. resting on the bottom G, and being held in its proper situation by the sides H and I, the end K at the time of observation is put into a short tube fixed to the Newtonian telescope, and may be turned about so as always to have the open part H I horizontal.

When the eye-piece Fig. 3. is screwed by its end M into the skeleton tube at L, Fig. 2. and the trough, Fig. 1. with any liquid to be tried is placed in the open part G H I, the sun's rays will come from the small mirror of the telescope to K, and passing through the plain glasses C D, inclosing the liquid, will enter the eye-piece M, and after the necessary refractions come to the eye at N. Any other single or double eye-piece of different magnifying powers may be screwed into L, instead of the piece Fig. 3; and the liquid may easily be tempered so as to intercept a proper quantity of light to suit every eye-glass which is in use, and thus to render the inspection of the sun perfectly convenient.

* Philos. Transf. 1801, p. 362, mentioned at page 21 of our Journal, but in that Number there was no room for the Plate.

XV. *On*

XV.

On the Effects of the Refpiration of the Nitrous Oxide; particularly an Inftance in which the Excitation of the Syftem produced unpleafant Symptoms. In a Letter from Mr. JAMES STODART.

To Mr. NICHOLSON.

SIR,

THE firft opportunity I had of breathing nitrous oxide was in the month of June laft, in company with Mr. Davy, in the laboratory of the Royal Inftitution. My curiofity had previoufly been much excited by what I had read and heard on the fubject. I began breathing rather fceptically, but my doubts were foon removed. A flight degree of giddinefs was followed by a pleafing thrilling and fenfe of warmth over the body; but not being ufed to breathe through a tube, and at the fame time a little alarmed, I fuffered a fmall quantity of common air to pafs into the lungs, and by thus diluting the gas its powerful action was fufpended. This firft attempt increafed my defire to breathe it in a pure ftate; and a few days afterwards I had another opportunity, in the company of the fame gentleman. After exhaufting the lungs by a forced expiration, and compreffing the noftrils, I began breathing as before. The air tafted diftinctly fweet; the flight giddinefs was, as before, quickly fucceeded by fenfations very difficult to be defcribed. I nearly loft fight of all fenfible objects, and felt as if in a moft delightful dream. On recovering from this trance-like ftate, my firft feeling was certainly a fentiment of pride or difdain.—I could with difficulty defcend to notice the objects around me. This emotion was fucceeded by an irrefiftible propenfity to mufcular action, in which I indulged myfelf, by walking or rather ftamping about the laboratory. It was in the morning that this experiment was made, and during the remainder of the day I felt highly exhilirated. In the evening I again breathed a quantity of the gas with nearly the fame confequences. I found it impoffible to refift the ftrong propenfity to active exertions. At night I went to bed as ufual, and after found fleep I awoke in the morning more than ufually chearful.

Effects of breathing the nitrous oxide.

Tafte of the gas. Giddinefs.

Delirium.

Recovery.

Irrefiftible propenfity to mufcular action.

No depreffion followed.

Other trials.

I have lately prepared and breathed the gas at different times, and the effects were always pleafurable. One circumftance I wifh to notice. A pain in the right fide of my face and head, which for fome weeks had been very troublefome, was certainly at firft increafed, after breathing four quarts of the gas. The pain, however, gradually leffened, and in about an hour was quite gone, nor had I any return of it either during the remainder of the day or following night. From this I began to fufpect that the gas had been inftrumental in fome manner in removing the pain. Some fymptoms of its return during the following day induced me to give this new agent a further trial. With this view I prepared fix quarts of the gas, and an hour before my bed-time I breathed it. I was foon wholly under its influence—totally loft to all fenfible objects—all was vifionary. I fancied a number of people in the room; imagined mufical founds; and the apartment appeared illuminated with varied coloured lights. A friend who was with me thinks I remained in this ftate a full minute after dropping the breathing-bag. My firft enquiries were wild and incoherent; but I gradually recovered my ufual temper of feeling, flept well during the night, and awoke free from pain in the face. I have felt very little of it ever fince. Whether the nitrous oxide was the removing caufe or not, I fhall not take upon me to determine. I ufed no other means; nor do I know whether any other perfon has breathed the gas under fimilar circumftances. A better acquaintance with this moft extraordinary agent would probably lead to important and ufeful difcoveries.

Pain in the face mitigated and removed.

Striking phenomena of deranged perceptions. Numbers of people—mufical founds—illumination of coloured light.

Gas breathed at the Royal Inftitution.

Since writing the above, I have again breathed the nitrous oxide in a very pure ftate at the Royal Inftitution, and was as ufual loft in pleafure. On recovering, I fignified a wifh to know the ftate of my pulfe, and was told it was above 140. This was on Saturday the 13th, about three o'clock. I continued under the ftrong influence of the gas as to mufcular action, &c. during the remainder of the day; but awoke early next morning with feelings very different to thofe I had formerly experienced. I felt a tremor, foon became faint, and this faintnefs increafed fo much, that for a fhort fpace of time I was as if finking into nothing. I was certainly under confiderable alarm. A mouthful of water recovered me from the apprehenfions of fainting. For fome hours afterwards I remained

Ufual effects. Alarming confequences next day.

remained in a very low ftate, during which I am certain that I repeatedly tafted the gas. But my mind was now perfectly tranquil, and at times I had feelings analogous to thofe I have fometimes experienced during the time of breathing the gas itfelf. During the whole of this and following day I felt depreffed, with occafional flight giddinefs; and as thefe feelings ftill continued, on the Tuefday morning I confulted Dr. Garnet, who confidering the excitibility of the fyftem to have been exhaufted by too powerful a ftimulus, directed a cordial medicine and a moderate quantity of wine, the good effects of which I foon felt. It required a few days, with nourifhing diet, to recover my former ftrength and fpirits. I never intend again to breathe the gas in fo large a quantity. I remain much at a lofs to account for the effects produced by the laft dofe, fo contrary to all my former experience; more efpecially as Mr. Davy does not think the dofe was at all extraordinary in quantity, and the gas was not different from that ufually prepared. It has ftood over water for a day, and was breathed at the fame time by Mr. Davy and another gentleman with no unufual confequence.

Extreme depreffion and occafional giddinefs.

Medical treatment.

I am, dear SIR,

With much refpect,

Your obedient Servant,

JAMES STODART.

Strand, Feb. 19, 1802.

P. S. I prepare the gas from the nitrate of ammonia; am particularly careful not much to exceed 400 degrees of heat, and have always fuffered it to ftand fome hours over water before it is ufed. I procure the falt in a very pure ftate from Mr. Accum.

XVI.

Remarks on the Processes for clarifying Liquids. By CITIZEN PARMENTIER *.

THE name of clarification is commonly given to a procefs by means of which liquids are deprived of fuch foreign fubftances as affect their tranfparency.

Clarifying is a procefs of fome importance.

This operation, however fimple it may be in appearance, deferves neverthelefs to be particularly attended to, efpecially when we confider the advantages it affords in the chemical and pharmaceutic arts. From thefe motives, I have thought it might be ufeful to communicate fome general notions refpecting the procefs.

Various methods.

It is not my intention to develope in this place the different methods made ufe of to obtain this purpofe, nor to fhew at length the effects they produce with regard to each of the objects fubmitted to the operation. It will be fufficient fimply to point out the principal phenomena attending operations of this kind.

Object of the procefs.

The purpofe to be anfwered, is, to clear the fluid from fuch bodies as, without being diffolved, remain fufpended, and impair its tranfparence and limpidity: but thefe bodies fometimes are feparated by repofe or filtration; in other inftances by the action of the air, of heat, of light, of motion, and of fermentation; and again in other inftances by means of agents, which, by uniting the fcattered particles in the liquid intended to be clarified, frequently change its nature, and no longer allow them to remain in their former ftate. We will begin our examination by attending to fpontaneous clarification.

Spontaneous clarifying of fluids

This takes place only when the particles to be feparated are decidedly of a lefs or greater fpecific gravity than that of the fluid in which they are fufpended. In this cafe they may unite at the bottom of the fluid, or at its furface, and form a magma, which is very eafily removed if the feparation has been complete. After this the fluid poffeffes all the tranfparence which can be defired, and the moft accurate filtration will add nothing to its clearnefs.

* Annales de Chimie.

This

This method of clarifying is sometimes subject to inconveniences, of which the principal are, that it requires much time, and tends by this delay to favour the formation of new products, which, by changing the composition of the fluid itself, no longer presents it, independent of the abstraction of the bodies which affected its clearness, the same as it was before its clarification. We find a very striking example of this kind when we consider what happens in the spontaneous clarification of the juices of plants or fruits. These juices when newly expressed are always turbid; they nevertheless become gradually clear; but their nature is not then entirely the same: they contain products which would not have been found if they had been clarified immediately after the expression. For this reason also it is that the juice of lemons, of gooseberries, &c. when examined before or after their spontaneous clarification, are so different in their taste, their colour, and their domestic utility.

affords time for other changes to take place;

In general we may consider it as a certain fact, that all the fermentable fluids are liable by spontaneous clarification to the effects here stated, whereas they do not take place with respect to such liquids as are little or not at all capable of fermentation, and of which the transparence is affected only by the interposition of particles incapable of acting in any manner upon the constituent parts of the fluids themselves.

and is not therefore suited to fermentable fluids.

Thus for example: Water, alcohol, ether, oil, &c. which in their first state may not have been perfectly transparent, may easily become so by spontaneous clarification, without the least change ensuing in their composition; for when we examine them after clarification, we find them in the same state as other similar fluids which have not been subjected to that process.

But other fluids are well adapted to this treatment.

The second process for clarifying fluids consists in filtering them; but this operation can never be performed without the assistance of intermediate bodies, of which the very contracted pores admit only of the passage of the fluid, and retain all the particles which were before suspended in it.

Filtration.

The instruments of filtration are exceedingly various. Paper, woollen cloths, linens, cottons, carded cotton, sponge, sand, earth, pounded glass, charcoal, porous stone, &c. all these bodies may be usefully employed in the present operation;

Different kinds of filters

tion; but their nature and their purity muſt firſt be examined more particularly when the fluid to be filtered is of a ſaline nature.

requireſelection. It is incumbent on the chemiſt to ſelect from among the different filters, that which, by producing the clarification of the fluid in the beſt manner, ſhall not at the ſame time effect any change in its conſtituent parts. Now the choice to be made in this reſpect muſt be determined from the knowledge we poſſeſs of the nature of the fluid, and that of the kind of filter proper to be employed.

Paper If the fluid be aqueous, vinous, alcoholic, or oily, paper may be uſed without inconvenience, provided it be of good quality. This laſt condition is abſolutely indiſpenſible, for without this the filtration would frequently prove defective.

acts mechanically. We know that paper is a *mechanical* texture of vegetable fibres, which have undergone different preparations. The particles of this fibre, by intertwining, leave pores, the tenuity of which is always governed by the ſtate in which the paſte or ſtuff exiſted at the moment when it was converted into paper. If this tenuity was conſiderable, the pores become ſpeedily obſtructed by the ſediment depoſited from the liquor under filtration, and at this period the filtration ceaſes. On the contrary, if the pores be very open, the filtration is made with rapidity in an incomplete manner, becauſe at the ſame time that the fluid paſſes through the pores it carries along with it the moſt minute particles which are ſuſpended, and there are only the coarſer which remain on the ſurface of the filter.

The great art is therefore to chooſe ſuch paper as has its pores of a requiſite magnitude to admit the fluid intended to be filtered, without ſuffering any of the particles which produced the turbidneſs to paſs through.

Different kinds of paper for filtering; Two ſorts of paper are found in commerce, which nearly produce this effect; and though they are not always as perfect as might be deſired, theſe have hitherto been preferred to every other. The one, which is of an imperfect white, is known in France particularly by the name of *papier Joſeph*. The other is a kind of grey paper, leſs coarſe than that which ſerves for wrapping up cheap goods. Neither of theſe have any ſize.

The

The white colour of the *papier Joſeph* ſhews that it has been manufactured with a ſuperior material to that uſed for the grey paper. The fluids which have paſſed through it are always very tranſparent, but it has the inconvenience of being eaſily torn, and its pores are ſoon obſtructed, ſo that its filtration is not effected with diſpatch.

The grey paper may be uſed for a long time alſo in producing clear fluids. But as the paſte out of which it was fabricated was not as pure as that of the *papier Joſeph*, it always communicates a diſagreeable taſte to the fluids, owing to the ſolution of the foreign matters it contains. On this account preciſely it is that certain fluids, ſuch as whey, wine, ratifia, and other liquids for beverage, when filtered through the grey paper, have always an odour and taſte, which delicate organs ſoon recognize. And hence it is that among thoſe fluids ſome of them are more ſuſceptible of alteration than when they have been filtered through the *papier Joſeph*. and their uſes.

The nature of the paper requires more particularly to be conſidered when ſaline ſolutions are to be filtered. If the grey paper be uſed, it frequently happens that part of its ſubſtance is diſſolved by the ſaline liquor, ſo that it becomes leſs pure than before. This inconvenience, which is not ſo perceptible when the *papier Joſeph* is uſed, may be ſtill further diminiſhed, by uſing the precaution of employing ſuch filters only as have been previouſly waſhed by repeated filtration of boiling water. The exact pharmacian ought to be always provided with filters thus waſhed, in order to have recourſe to them upon occaſion. Joſſe, ſo diſtinguiſhed in pharmacy, and to whom we are indebted for many important obſervations, has found the advantage of theſe inſtruments under a multitude of circumſtances. He has, among other obſervations, remarked, that whey clarified and filtered through *(du papier raiſin)* may be kept for more than fifteen days, by filtering it daily, which could not be done if it had been filtered through the common grey paper, even though previouſly waſhed.

By an effect directly contrary to this, different juices and plants are preſerved tranſparent, and in good condition, without paſſing to the acid ſtate, in conſequence of their having been daily filtered through the grey paper. It was obſerved merely that their colour became more intenſe on the firſt days, and that they afterwards became inſenſibly diſcoloured.

But

Various precautions.

But while we confider the nature of filters, their form and fituation are no lefs important. In order that a filter of paper may produce its full effect, it muft not adhere by its whole furface againft that furface which fupports it, for if this were the cafe the filtration would be foon ftopped. This inconvenience is avoided by dividing it in different directions. But as the folds are foon flattened, fome prefer placing between the fupport and the filter, ftraws or fimple tubes of glafs. I muft confefs that this laft method has not always fucceeded with me, and that I have moft commonly obferved, that the folds made in filters produce nearly as much effect as the pieces of ftraw and tubes. In Germany (and in England) they have funnels grooved or fluted within for this ufe.

Whatever may be the precautions which have been taken, a period will arrive at which the filtration becomes flow, and at length totally ftops. This effect takes place when the pores of the paper are fo much obftructed, that they no longer admit the paffage of the fluid. Sometimes the filtration may be prolonged by giving a flight circular motion to the funnel; but this effect is of fhort duration, and there is no other remedy but to change the filter itfelf. It feems that there has not yet been found any means of remedying this inconvenience, which is common to all filters whatever.

Filtres of woollen;

We have before obferved that filters are made of woollen cloth, of piece goods, and of carded cotton. The woollen were formerly much ufed, and were even the firft filters adopted. They were formed into the figure of a cone, of which the bafe was kept open by an hoop that was fixed in a frame with fupporters. This kind of filter is ftill ufed to filter ratafias, as it may be rendered very capacious, and is fufceptible of receiving a large quantity of fluid at once; but it affords little, and it is therefore neceffary to wait a long time before the fluid paffes clear; for which reafon it is feldom ufed, excepting when no other apparatus can be procured.

and their ufes.

Neverthelefs, when the filtration of fyrups is required, woollen cloths are ufed; but then inftead of giving them the form of a cone, the operator fimply fixes his cloth in a fquare frame, faftening the four corners upon pins difpofed for that purpofe. The boiling fyrup is poured in the middle, where it always forms a kind of concavity, and frequently after a few minutes the liquid paffes very clear.

This filter may be ufed for many other fluids, particularly thofe of an aqueous nature, which do not contain potafh or foda in folution. For if they were only flightly alkaline, the filter would be foon deftroyed, and the filtered liquor would not poffefs the requifite qualities.

Linen or cotton cloths, and paper, are commonly ufed for alkaline liquids, and they fucceed very well, particularly when thofe fluids are not too concentrated. With regard to carded cotton, it is referved to filter fuch fluids as are of confiderable price, or very fcarce. Linen or cotton cloths, or paper, to what liquids applied.

This filter is made by introducing carded cotton into the tube of a glafs funnel, where it is lightly preffed together with a glafs rod fo as to form a kind of ftopper; after which the fluid intended to be filtered is poured into the funnel. The filtration takes place drop by drop and after the firft drops are feparated, thofe which follow are always clear. The effential oils may be very well filtered by this method, without any fear of that lofs which would neceffarily follow if the other filters before fpoken of were ufed. Carded cotton.

The acids, particularly thofe which are concentrated, can only be filtered through pounded glafs; but care muft be taken not to ufe this fubftance till after it has been wafhed feveral times, firft in a large quantity of water, and afterwards in an acid, in order to deprive it of the earthy fubftance which the acids might diffolve. Filter of pounded glafs;

Glafs filters may be very well conftructed in a funnel. The great art in order that they may produce the effect, is to fix in the firft place fome fragments of glafs in the tube, and afterwards to have others which are fmaller, and this procefs muft be continued, conftantly diminifhing the fize of the fragments until the powder lies to the thicknefs of three or four inches, the laft ftratum of which muft be very finely pulverifed. how conftructed.

This kind of filter operates fo well, that in lefs than an hour it is poffible to filter through a funnel of middling fize feveral kilograms of acid.

Sand is alfo very commonly employed to clarify water for domeftic ufes. Sandy fprings are in fact true filters, of which the effect is more certain, in proportion as the layers of fand are fo difpofed, that the water upon them may be obliged to pafs through the fand fucceffively, and leave thofe bodies which injure its tranfparence. Filtring by fand.

The

The art of constructing fountains or springs with sand is not yet carried to the degree of perfection it is capable of, and though it may seem to be an object of little importance, it well deserves to fix the attention of philosophers.

Observations. Experience also shews, that filters or fountains of sand cannot be used with success but for a limited time. It is necessary to change the sand frequently, or at least to wash it, in order to deprive it of the earthy heterogeneous substance, deposited by the water, and which when accumulated to a certain point would not only oppose or diminish the filtration, but also communicate to the fluid a flavor, which is the more disagreeable in proportion to the length of time it remains in contact with it. Nothing is more easy, as is well known, than to deprive river water of the earth which it suspends, and which obscures its transparence. For this purpose it requires only to be left a few hours in an earthen vessel uncovered, because the action of the air is necessary to effect this precipitation speedily and completely.

(To be continued.)

SCIENTIFIC NEWS.

Extract of a Letter from Brunn, in Moravia, dated January 3, 1802.

Tellurium is asserted to be antimony only. A CHEMIST of Vienna pretends to have observed, that the new metal *tellurium*, discovered by Klaproth, and generally acknowledged as such, is nothing else but *regulus of antimony*: an observation which, according to my own experiments, has much probability.

Water said to be decomposed by magnetism. Galvanism is at present a subject of occupation of all the German philosophers and chemists. Tromsdorff has burned various metals by means of a pile of 150 plates; and at Vienna a discovery has been made, that an *artificial magnet*, employed instead of a Volta's pile, decomposes water equally well as that pile and the electrical machine; whence (as they write) the *electric* fluid, the *galvanic* fluid, and the *magnetic* fluid are the same. *

* I applied the poles of a five bar magnet to two steel wires in the tube of water, having their extremities in the water less than one-tenth of an inch asunder, *and perceived no effect*.---W. N.

On

On the Muriatic Acid.

Chenevix on the ftates of the muriatic acid.

The reading of Mr. Chenevix's paper upon oxigenifed and hyperoxigenifed muriatic acid, before the Royal Society, was concluded on Thurfday the eleventh.

After a fhort account of the experiments that had been made before him, and particularly of the ingenious conjecture of Mr. Berthollet,* he ftates the means by which he has afcertained, that the acid contained in hyperoxigenifed muriate of potafh is muriatic acid, in a particular ftate of combination with oxigen; and the experiments by which he determined the proportion of thofe elements. From the quantity of oxigen and of fimple muriatic acid contained in the falt, he proves that hyperoxigenifed muriatic acid confifts of

Oxigen	65
Muratic acid	35
	100

From the proportions of the falt which is formed when a current of oxigenifed muriatic acid is paffed through a folution of potafh, and which he found to be compofed of the fame elements, and in the fame proportion as oxigenifed muriate of potafh would be, if at the very moment of its formation it had not been refolved into fimple and hyperoxigenifed muriate, he concludes that oxigenifed muriatic acid is compofed of

Oxigen	16
Muriatic acid	84
	100

From a number of experiments, which are ftated at length, Mr. Chenevix imagines that the falts of the genus oxigenifed muriate, do virtually exift; but that by fuperior affinities they are refolved into muriate and hyperoxigenifed muriate at the very moment that the acid comes into contact with the bafes.

He defcribes and analyfes the falts of the genus hyperoxigenifed muriate; and mentions the hyperoxigenifed muriate of ammonia as an extraordinary inftance of difpofing affinities.

* Journal de Phyfique, 1788, page 217.

He

Chenevix on the salts of the muriatic acid.

He obtained the alkaline falts pure by repeated cryftallifations, and the earthy falts by boiling them with phofphate of filver.

He has examined likewife feveral of the metallic falts of this genus, and mentions hyperoxigenifed muriate of filver, as ftrongly marking the difference between muriatic and hyperoxigenifed muriatic acid. He afcribes to this falt, when mixed with combuftible bodies, an expanfive force, which he thinks he will be much within bounds, if he ftates to exceed five times that of any known detonating falt.

He concludes with an appeal to the chemical world, whether, in the prefent ftate of the fcience, it would not be more philofophical to fay,

Muriatic radical, or fome one word analogous to it,	Inftead of	Muriatic acid.
Muriatous acid,		Oxigenifed muriatic acid.
Muriatic acid,		Hyperoxigenifed muriatic acid.

and ftates the arguments in favour of either mode of appellation.

As a warning to thofe who would repeat his experiments he relates, in the courfe of his paper, an accident that happened in his laboratory to himfelf and to Mr. Vandier, by which the latter gentleman had almoft loft his fight, and was wounded in the moft dreadful manner.

A new Method of obtaining the Gallic Acid pure. By M. FIEDLER, *Student in Pharmacy* *.

As alumine has a great affinity with many vegetable fubftances, I inferred that this property might be applied to difengage the gallic acid from the extractive and the tanin which always adhere to it; and by that means to obtain the acid pure and eafily cryftallizable.

Solution of alum precipitated by potafh.

Experiment 1.—I diffolved two ounces of alum in boiling water; I filtrated and precipitated it by a folution of potafh; I feparated the precipitate, and edulcorated it till the water of the lixiviation no longer rendered the muriate of barytes turbid.

* Journal de Chimie, par Van Mons. I. 85.

Experiment

Experiment 2.—I infufed, to a reduction of one half, an ounce of good gall nuts in fixteen ounces of water. The refult was a brown infufion, which after many filtrations continued turbid.

Nut-gall infufed in water.

Experiment 3.—I mixed with the preceding infufion the precipitate of alumine, and frequently agitated the mixture with a glafs rod. The next day I filtered, and obferved that the matter paffed perfectly clear, which convinced me that the alumine had precipitated the extractive. I wafhed the precipitate with warm water, until the water no longer blackened fulphate of iron, after which I left it to dry.

The precipitate of alum, being added to the infufion, combined with the extractive part.

Experiment 4.—As the alumine had evidently feparated the extractive fubftance from the infufion of gall nuts, I could only fuppofe that the liquor of the filtration muft contain tanin at the fame time with the gallic acid. I afcertained the prefence of the latter, by a folution of fulphate of iron; and to difcover the former, I proceeded in the following manner.

The clear fluid might contain tanin and gallic acid.

Experiment 5.—I diffolved two fcruples of ifinglafs in an ounce of water, and poured a few drops of this folution into the liquor of the gallic acid; they did not make it turbid in the leaft; which proved that it did not contain any tanin, which, with the gelatine, ought to have formed an elaftic fubftance, infoluble in water. As the tanin was not difcoverable in this liquor, there remained no doubt of its having combined with the alumine, which I afcertained in the following manner.

But, by the teft of ifinglafs, it contained no tanin. Confequently the tanin was combined with the alumine of Exp. 3.

Experiment 6.—I digefted a portion of the precipitate of the infufion of gall nuts with diluted fulphuric acid. It was entirely diffolved. The liquor paffed clear through the filter, but coloured by the extractive matter. I dropped in a little of the ifinglafs folution, and immediately obferved white filaments floating in the liquor, which by agitation formed into a mafs, poffeffing all the properties of tanned glue; namely, of becoming elaftic by heat, of foftening by water, infolubility in alcohol, &c.

Precipitate of No. 3. was diffolved in fulph. acid, which feized the alumine, while the tanin was precipitated by mucilage.

Experiment 7.—The filtered liquor, as well as the water of the lixiviation in the third experiment, of courfe held in folution nothing but the gallic acid. I concentrated the two fluids by flow evaporation, and obtained a falt in fine needles, which was the pure gallic acid.

The clear folution of gallic acid, Exp. 4. afforded fine cryftals of gallic acid.

The

The Seven following Articles are Extracts of a Letter of CIT. VAUQUELIN *to* CIT. VAN MONS *.

1. *Discovery of the Chromate of Iron in France.*

Chromate of iron in France.

The chromate of iron has been found in abundance in the department of Var: we can now prepare any quantities of this metal, and study its properties in detail more than we have hitherto been able to do. Above all, we can compose in a direct way the chromate of lead, which is a very interesting object in painting. The chromate of iron may also be used to form a beautiful green on porcelain or artificial stones.

2. *Discovery of the Emerald.*

Emerald in France;

In a mineralogical tour which Cit. Lelievre, member of the Council of Mines has just made, he has discovered a quarry of emeralds, which are so abundant, that they are used in the country to pave the roads. They are found in the environs of Limoges mixed with granite, often without any regular form, and sometimes crystallized, but their colour and transparency are not beautiful.

3. *Discovery of the Neutral Phosphate of Iron.*

and neutral phosphate of iron.

Cit. Lelievre has likewise discovered in the same place a mineral sufficiently interesting, namely, a perfect phosphate of iron, that is to say, that the iron is intirely saturated by the phosphoric acid, a combination which had not hitherto been found in nature; it has a red brown colour, is semi-transparent, and has a foliated texture.

4. *On the Zoonic Acid.*

The supposed zoonic acid is the acetous;

Cit. Thenard has just finished an examination of the zoonic acid, in which he appears to have demonstrated that it is nothing but the acetous acid combined with a peculiar animal matter.

5. *On the Cobaltic Acid.*

and the cobaltic acid is arsenic.

Cit. Darracq has read a paper to the Institute, in which he shews that the acid found in zaffre by Cit. Brugnatelli, and which he named the cobaltic acid, is absolutely nothing but the arsenic acid.

You see that by degrees the number of chemical substances diminishes, which is a great advantage to the science.

* Journal de Chimie, par Van Mons. I. 218.

ACCOUNT

ACCOUNT OF BOOKS OF SCIENCE.

Elements of Chemiſtry. *By* J. Murray, *Lecturer on Chemiſtry, &c.* Edinburgh, 1801. Longman and Rees, London.

THE author of this work appears to be well acquainted with every thing at preſent known relative to the ſcience of chemiſtry. He follows the claſſification of ſubſtances according to the order of their ſimplicity, and has given a digeſted, conciſe, and perſpicuous detail of every important fact concerning their chemical relations. Murray's elements of chemiſtry.

The view he takes of the preſent ſtate of our knowledge reſpecting the important ſubject of heat is clear, candid, and ſatisfactory. On this, as well as on other controverted points, after ſtating the leading arguments on both ſides he preſents his own opinion where requiſite, in the modeſt form of a query. On the whole, this work may be conſidered as a ſafe and clear guide to the ſtudent of chemiſtry; and as an uſeful courſe previous to the ſtudy of the more extenſive ſyſtem of Fourcroy. B. B.

A Syllabus of a Courſe of Lectures on Natural and Experimental Philoſophy. *By* Thomas Young, *M. D. F. R. S. Profeſſor of Natural Philoſophy in the Royal Inſtitution of Great Britain.* Quarto, 193 pages. From the preſs of the Royal Inſtitution, where the work is ſold, and at Cadell's, in the Strand, London.

This ſyllabus appears to be one of the moſt complete and accurate outlines of ſcience which has been hitherto publiſhed. It is divided into four parts, and each of theſe into ſubordinate ſections, conſiſting of paragraphs regularly numbered. I cannot pretend in this notice to enter into any diſcuſſion of the numerous ſubjects which, in the nature of things, muſt be very conciſely ſtated in ſuch a work, and of which the illuſtrations muſt be ſought in the lectures of the learned profeſſor; but ſome intimation of the arrangement and objects of inſtruction will be gathered from the titles as follow. Part I. Mechanics. Of induction; motion; compoſition of motion; centre of inertia and of momentum; accelerating forces; central forces; projectiles; motion in given ſurfaces; colliſion Young's ſyllabus of philoſophical lectures.

Young's fyllabus of philofophical lectures.

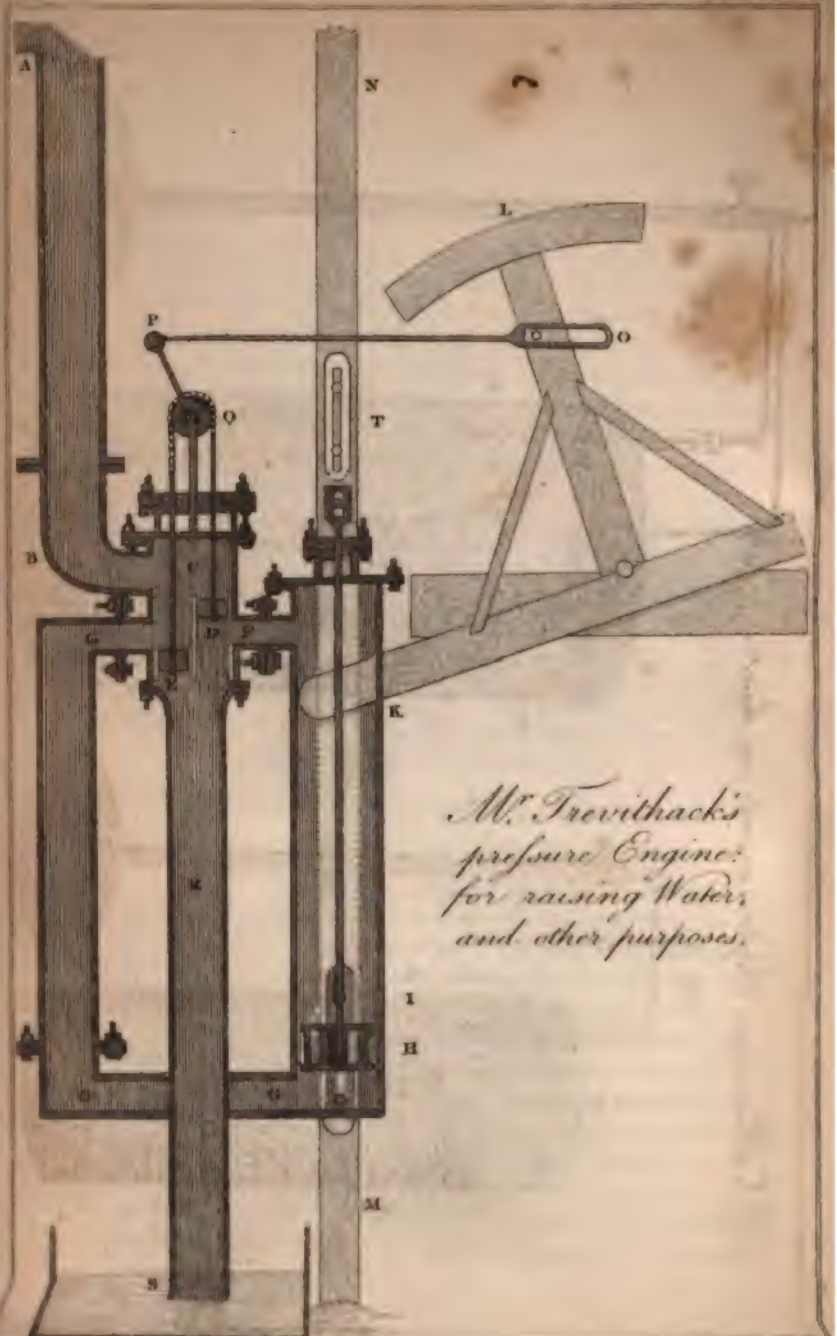
and energy; preffure; equilibrium; rotatory power; preponderance; practical mechanics in general; geometrical inftruments; perfpective; ftatics; friction; paffive ftrength; architecture; carpentry; wheel-work; union by twifting; economy of motion; time-keepers; raifing weights; friction wheels; carriages; compreffion and extenfion; penetration and attrition; trituration and demolition. Part II. Hydrodynamics. Of hydroftatic equilibrium; floating bodies; fpecific gravity; pneumatic equilibrium; hydraulics; refiftance of fluids; hydraulic machines; pneumatic machines; found; harmonics; properties of light; dioptrics and catoptrics; optical inftruments; phyfical optics; nature of light. Part III. Phyfics. Of the fixed ftars; the fun; primary planets; fecondary planets; comets; laws of gravitation; fenfible effects of the celeftial motions; practical aftronomy; geography; tides; general properties of matter; heat; electricity; magnetifm; meteorology; natural hiftory. Part IV. Mathematical Elements. Of quantity and number; fpace; the comparifon of variable quantities; the properties of curves; practical rules and tables.

The whole work is beautifully printed, and fuch demonftrations as occur are diftinguifhed from the narrative or enunciation by a fmaller type. The figures are extremely neat. A fifth part is mentioned as probably hereafter to appear, containing a catalogue of the beft authors.

Mr. BLAIR's POPULAR LECTURES.

Mr. Blair has recently commenced a courfe of popular lectures, at the Bloomfbury Difpenfary, for the information of fcientific perfons, amateurs of natural hiftory, and ftudents in the liberal arts. He propofes to explain and illuftrate the following fubjects, on Tuefday evenings, at eight o'clock:

On the component parts of the body—the bones, cartilages, and ligaments—mufcles, and mufcular action—the integuments and membranes—brain, nerves, and fenfation—the heart and vafcular fyftem—the blood, circulation, and abforption—glands, fecretions, and excretions—refpiration and animal heat—digeftion, nutrition, and growth—utero-geftation, and parturition—the eye, and phenomena of vifion—functions of the ear, nofe, and mouth—phyfiognomy, beauty, and the paffions.

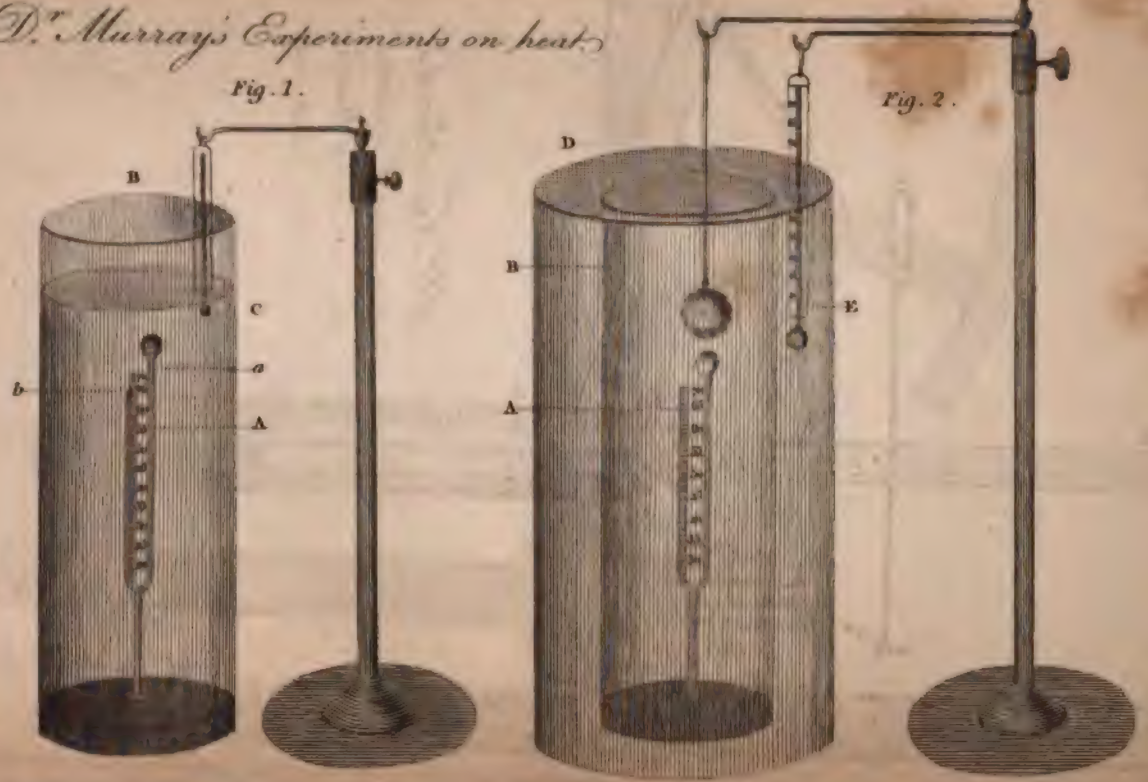


Mr. Trevithack's pressure Engine: for raising Water, and other purposes.

Dr. Murray's Experiments on heat

Fig. 1.

Fig. 2.



Liquids substituted instead of dark Glasses by Herschel.

Fig. 1.

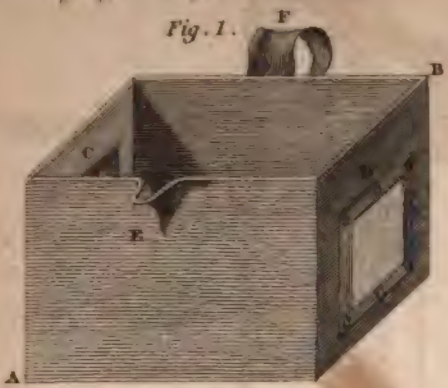


Fig. 2.

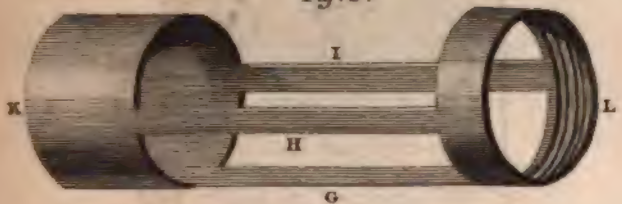
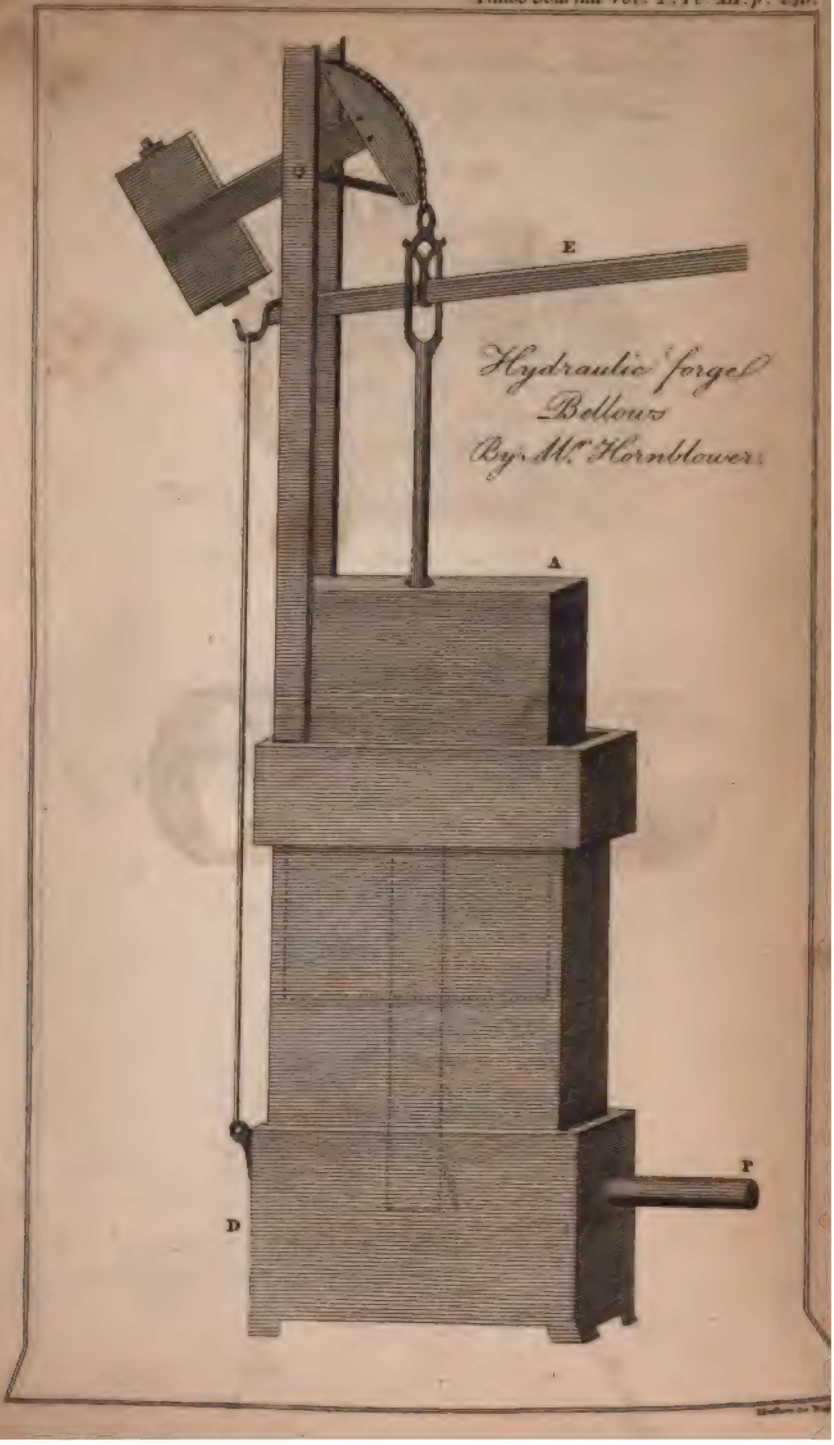


Fig. 3.



Hydraulic forge Bellows
By Mr. Hornblower.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1802.

ARTICLE I.

Experiments on the Tranſmiſſion of Heat downwards through Mercury and through Oil contained in Veſſels of Ice; by which thoſe Fluids are proved to be proper Conductors of Heat. By Mr. John Murray, *Lecturer in Chemiſtry, Materia Medica, and Pharmacy, at Edinburgh. Communicated by the Author.*

IN a former Memoir * I related ſeveral experiments made with the view of determining the queſtion, whether fluids are capable of conducting caloric, and ſtated the ſources of fallacy by which any ſtrict concluſion from theſe is invalidated. The experiment calculated to determine this queſtion with greateſt certainty is that of heating a fluid downwards by bringing a hot body into contact with its upper ſurface; but in making this experiment a ſource of error is always preſent in the conducting power of the veſſel in which the fluid is contained. When the upper portion of fluid has its temperature raiſed by the application of a hot body, part of it muſt flow towards the ſides of the veſſel, and give out part of its caloric; the ſolid

Review of the experiment of tranſmitting heat downwards through a fluid.

* Philoſ. Journal, I. 165. where by miſtake the author's titles are erroneouſly given.---W. N.

matter of the vessel must convey more or less of this caloric downwards, and communicate it to the fluid beneath; and thus caloric may appear to be conducted to a considerable depth, though the fluid do not actually possess a conducting power. In any usual mode of performing this experiment, this source of fallacy cannot be intirely obviated; nor can its effects be estimated with such accuracy, as to determine precisely how far any observed rise of temperature in a fluid is to be ascribed to its operation.

Error from the vessel, avoided by forming it of ice.

It occurred to me, that this error might be completely avoided by employing a hollow cylinder of ice to contain the fluid. Suppose a thermometer to be placed horizontally within such a vessel, its bulb being in the axis of the cylinder, and covered with a fluid at the temperature of 32°. Let a hot body be brought in contact with the upper surface of the fluid, or even immersed in it, but still so as to be at some distance from the bulb of the thermometer; if the thermometer rise it may be concluded that the fluid has a conducting power, since the caloric could reach the bulb in no other mode. Caloric does not pass through fluids by radiation; it is easy to perform the experiment so as to prevent the thermometer from being heated by any motion of the fluid, from immersing the hot solid; and lastly, no caloric could be transmitted by the sides of the vessel, since ice cannot have its temperature raised above 32°, and cannot therefore communicate any temperature above that to a contained fluid. The conclusion therefore seems undeniable, that if in such an experiment the thermometer rise, caloric must be conveyed to it by the fluid interposed between it and the heated solid.

If the thermometer do rise the fluid is a conductor.

Whether the opposite conclusion may be affirmed, in case the thermometer do not rise?—No.

It seemed doubtful, however, whether the converse of the proposition would hold equally good. If the thermometer were not to rise, would this prove that the fluid is a perfect non-conductor? It is not evident *a priori*, whether a fluid contained in a vessel of ice is capable of being heated above 32°, or at least could convey caloric above that temperature to any perceptible distance, even allowing it to have a conducting power. Suppose that the heated solid is immersed in the fluid, the particles in contact with it will be heated, and will form an ascending current which must flow towards the sides of the vessel. The ice will abstract the excess of caloric from these heated particles, and they will return to the tem-

perature

perature of 32°. It appears doubtful, therefore, whether any part of the fluid beneath the heated folid, fuppofing it to be capable of conducting caloric, can have its temperature in fuch a fituation raifed; and hence it appears uncertain, whether from the circumftance of the thermometer not rifing in fuch an experiment, it could fairly be concluded, that the fluid is a perfect non-conductor of caloric.

If the heat be given out fafter than the ice can abforb it, the thermometer will rife *if the fluid be a proper conductor.*

There are alfo reafons, however, which render it probable, that any conducting power in the fluid if it do exift, may be difcovered by an experiment of this kind, by the rife of the thermometer. Suppofing the fluid to poffefs fuch a power, it is fufficiently poffible that the caloric may be conveyed from the heated folid through the interpofed fluid, (efpecially if the quantity of the fluid be fmall) to the thermometer, more quickly than it can be cooled by the ice. Two facts render probable this fuppofition. 1ft. Ice is an imperfect conductor, and therefore abforbs caloric flowly from another body. If a piece of ice be thrown into water at 40 or 50°, a confiderable time elapfes before the temperature of the fluid is reduced to 32°; and 2dly. The celerity with which a hot body gives its caloric to a cold one, is, *cæteris paribus*, proportioned to the difference between their temperatures. If the hot body, for example, have a temperature 100 degrees higher than the cold one, it will communicate caloric to it with much more celerity, than if it were at a temperature only five degrees higher. This caufe operates in the prefent experiment. The difference between the temperature of the heated folid fufpended over the thermometer, and that of the fluid interpofed is very confiderable, and it will therefore give caloric to it rapidly. On the other hand, the difference between the temperature of the particles of the fluid that are heated, and that of the ice with which they come in contact muft be much lefs, and therefore they will part with their caloric more flowly. From this circumftance, independent of the flownefs with which ice abforbs caloric, the communication of caloric in this experiment muft be more rapid than its abftraction; and therefore if the fluid do poffefs a conducting power, the thermometer muft be heated.

Whether the ftratum of heated fluid can extend below the hot heated body, and thus

In reflecting on this fubject it feemed even poffible, that the fluid might be cooled fo much more flowly by the fides of the veffel, than it was heated by the folid fufpended in it, that the

raife the thermometer?—Not unlefs the veffel conducts; which in this cafe it does not.

heated particles might accumulate, and perhaps at length come in contact with the bulb of the thermometer, raife its temperature, and thus give the appearance of the fluid poffeffing a conducting power, though it had none. I was foon fatisfied, however, that this could not be the cafe. Suppofing the caloric to be ever fo flowly abftracted from the hot particles, ftill the ftratum of fluid directly heated, can never extend beneath the folid which is the fource of the caloric. It is from it that the current of heated particles afcend, and they defcend by the fides of the veffel, where their temperature may be reduced partially or intirely. But this defcending current cannot proceed lower than the point where the afcending one commences, and of courfe no part of the fluid which has thus been directly heated can reach the bulb of the thermometer. In glafs veffels this is not the cafe, becaufe in thefe caloric is given out by the fides of the veffel, even beneath the heated folid, which is fufpended within it. But in an ice veffel there is no communication of this kind, as it cannot conduct caloric at any temperature above 32°.

But if the thermometer rifes, the fluid conducts; and if not, the fluid is probably a nonconductor.

The experiment then which has been ftated is fubject to none of thefe fources of fallacy. If the thermometer rife, there can be no doubt that the fluid conducts caloric. If it do not rife, it may be concluded, if not with equal certainty, at leaft with the greateft probability, that it has no conducting power.

A veffel was formed of ice.

A quantity of water was frozen in a tin mould, fo as to form a hollow cylinder A, Pl. XIII. Fig. 2. the diameter of the cavity of which was three inches, and the depth $3\frac{1}{2}$ inches, the thicknefs of the ice forming its fides and bottom, being $1\frac{1}{2}$ inch. A thermometer B was introduced into it horizontally at the depth of one inch, the bulb being in the axis of the cylinder, and was frozen in, fo that the veffel was capable of containing any fluid. It is evident that by pouring a fluid into this ice cylinder, fo as to cover the bulb of the thermometer to a greater or lefs height, and by fufpending in it a heated folid, any propagation of caloric through that part of the fluid beneath the folid, will be afcertained by the rife of the thermometer.

Water could not be ufed in thefe experiments, becaufe it contracts by heat

For making this experiment with a view to determine the conducting power of fluids, water is altogether unfit, from the fingular property it has of expanding inftead of contracting in every reduction of temperature from 40° of Fahrenheit to 32.

Suppofe

Suppofe the cylinder of ice to be filled with water at 32°, and a heated folid to be fufpended in it, the portion of water in contact with the folid, will immediately have its temperature raifed perhaps two or three degrees. But by this increafe of temperature it is not expanded, but is contracted in volume, it becomes heavier, and muft therefore fink in the fluid towards the bulb of the thermometer; it will be fucceeded by another heated portion, a defcending current muft thus be formed, and the thermometer will have its temperature raifed by the contact of particles which had been directly heated by the fufpended folid.

near 32°, and would defcend to the thermometer.

Oil and quickfilver are not liable to this fource of fallacy. It has been afcertained by Crawford and De Luc, that the diminutions of volume in thefe fluids from decrements of temperature are nearly uniform, or at leaft at any temperature neceffary for performing experiments of this kind they contract with fufficient regularity, as is indeed fufficiently fhewn by their ufe as thermometrical fluids——With thefe fluids therefore the following experiments were made:

Oil and mercury do not contract by heat; at leaft about the temperature here mentioned.

Experiment I. A quantity of almond oil was poured into the ice veffel, fo as to cover the bulb of the thermometer one quarter of an inch. A fmall iron cup C, cylindrical, two inches in diameter, and flat in the bottom, capable of holding two ounces by meafure, was fufpended fo as merely to touch the furface of the oil, and was filled with boiling water. This was preferred to a folid ball, as prefenting a larger furface to the oil, and as it was more eafy to afcertain the precife depth at which it was immerfed. At the beginning of the experiment the thermometer ftood at 32°. In a minute and a half it had rifen to $32\frac{1}{4}$, in three minutes to $34\frac{1}{2}$, in five minutes to $36\frac{1}{4}$, in feven minutes to $37\frac{1}{2}$. At this point it became ftationary, having rifen $5\frac{1}{2}$ degrees of Fahrenheit's fcale in feven minutes. The temperature of the water in the cup had in this time fallen to 96°. The thermometer, after remaining ftationary at $39\frac{1}{2}$ for fix minutes began to fall, and it continued to defcend at the rate nearly of a degree in a minute and a half, till it returned to 32. At the conclufion of the experiment the fides of the cylinder at the upper part were partly excavated, the excavation was greateft at the furface of the oil, it became lefs as it defcended, but it had taken place in a flight degree, even lower than the bulb of the thermometer,

Experiment I. Almond oil at 32° in the ice veffel was heated by water poured into a ftationary metallic veffel flightly immerfed in it. *The heat defcended* to a thermometer.

Excavation of the ice.

meter, fhewing that the fluid had been heated to this depth. The oil ftill covered the thermometer one quarter of an inch.

Exp. II. Repetition with a greater depth of oil.

Experiment II. The experiment was repeated, the oil covering the thermometer half an inch. The temperature* was as formerly 32°, in three minutes the thermometer had rifen to $32\frac{1}{4}$, in fix minutes to $32\frac{3}{4}$, in eight minutes to 33, in 12, to 34, in 15 minutes to $34\frac{1}{2}$, at which it became ftationary. In this experiment, therefore, the thermometer had rifen $2\frac{1}{2}$ degrees in 15 minutes: it defcended as flowly as in the preceding experiment. In a third experiment in which the thermometer was covered three quarters of an inch with oil, it rofe only $1\frac{1}{2}$ degree in the fame time.

Exp. III. Repetition with mercury. Speedier tranfmiffion of heat.

The fame experiment was next repeated with quickfilver in a fimilar cylinder of ice. 1ft. The quickfilver covered the bulb of the thermometer one quarter of an inch. The fmall iron cup was not fufpended, but allowed to float on its furface, and was filled with boiling water gently poured in. The thermometer began to rife inftantly: in one minute it had rifen from 32° to 36°, it remained at that temperature, or at leaft with the increafe of half a degree, for another minute, and then began to fall; in three minutes it had fallen to 35, the temperature of the water in the cup having fallen to 102. The thermometer continued to defcend flowly, till it returned to 32°.

Whether the fubfidence of the mercury from fufion of the ice can affect the conclufion?

In this experiment there is a particular fource of fallacy, againft which it was found neceffary to guard. From the melting of the fides of the cylinder of ice at the upper part by the contact of the heated fluid, the diameter of the cavity is enlarged, and therefore the column of mercury muft diminifh in height. In the experiment with the oil this does not take place, becaufe the water formed from the melting of the ice being heavier, falls by the fides to the bottom, and fupports the column of oil at its precife height, the one circumftance counterbalancing the other, fince as much ice as is melted, as much water is produced, and very nearly the fame volume is occupied by both. It was accordingly afcertained by exact meafurement at the end of each experiment with the oil, that it had not funk perceptibly; in other words, the bulb of the thermometer remained covered with the fame height of fluid, as at the commencement of the experiment. But with the quickfilver the cafe is otherwife; the water produced by the melting

melting of the ice floats on its furface, and hence from the enlargement of the diameter of the veffel, the height of the column of mercury muft be diminifhed. It was therefore neceffary to afcertain with accuracy, whether this diminution had proceeded fo far, as that the particles of mercury which had been in contact with the bottom of the iron cup, and directly heated by it, had come in contact with the bulb of the thermometer. This was found not to be the cafe. At the end of the experiment the bulb was ftill covered with fo much mercury that the cup floated freely; and it was afcertained in particular both by actual meafurement, and by a gage frozen in the fide of the cylinder, that the quantity of mercury covering it was ftill one-eighth of an inch deep. As therefore the cup merely floated on the furface, the part of the fluid *directly* heated by it could not have come in contact with the bulb. This was likewife clearly eftablifhed by the phenomena of the experiment itfelf; the thermometer rofe rapidly at the commencement of it before the quickfilver could have funk fenfibly, and it remained ftationary at the end when the full defcent had taken place, although the water in the cup was ftill warm. On pufhing it down fo as to caufe it to touch the bulb the thermometer rofe rapidly, a proof that it had not previoufly been in contact with it. In this point of view therefore the experiment was unexceptionable, and thofe which follow were ftill lefs liable to any fallacy of this kind; as a larger quantity of fluid was interpofed between the heated body and the thermometer.

Proofs that it did not.

2d. As much mercury was poured into the veffel of ice as to cover the bulb of the thermometer half an inch, and water at 212° was poured into the iron cup floating on its furface. The thermometer ftood at 32, in one minute it rofe to $32\frac{1}{4}$, in two minutes to $33\frac{1}{4}$, in three minutes to $33\frac{1}{4}$, it then became ftationary, and in fix minutes more began to fall. The quickfilver in the veffel was found to have funk rather lefs than one quarter of an inch. The thermometer therefore at the end of the experiment ftill remained covered with one quarter of an inch of fluid.

Exp. IV. Repetition with a greater depth of mercury.

3d. When the experiment was made a third time, the bulb of the thermometer being covered with one inch of quickfilver, the rife of temperature amounted to three-fourths of a degree.

Exp. V. and ftill greater.

All

Variation of the circumstances.

All thefe experiments were frequently performed, they nearly agreed, and the above are the average refults. Every precaution was taken to enfure accuracy, in which I was affifted by feveral friends. They were likewife occafionally varied. In one inftance the cylinder of ice was made fo wide, that a fmall thermometer was placed entirely within it in a horizontal pofition, and covered with oil, but the refult was nearly the fame. In others a folid brafs ball was fufpended in the fluid inftead of the cup with hot water, but without any material difference in the refult. In conftructing the veffel it was found neceffary that the ice fhould be frozen hard, that it might contain the fluid without allowing any of it to tranfude; and in the experiment with the mercury the external mould of tin was allowed to remain round the ice cylinder to give it ftrength, and enable it to contain the mercury without any rifk. This however could make no difference in the experiment, and the refult was found to be the fame when the tin was removed.

Developement of the procefs.

In all thefe experiments then, a rife of temperature in the thermometer took place, greater or lefs, proportioned to the quantity of fluid interpofed between it and the heated body, From the nature of the experiment this rife could not be expected to be confiderable, fince the fluid when heated muft quickly have had its caloric abftracted by the cylinder of ice in which it was contained. Hence at a certain ftage of the experiment the thermometer became ftationary, though the heated body fufpended over it ftill retained a confiderably high temperature, the communication of caloric to the fluid not being equal to its abftraction by the ice; and when the communication became lefs rapid than the abftraction, the thermometer began to defcend. In all thefe experiments, however, even in thofe in which the largeft quantity of fluid was interpofed, the rife of the thermometer was unequivocal, and in fome of them, confidering the circumftances of the experiment, very confiderable.

Conclufion. *The fluids were direct conductors of heat.*

This rife it appears to me impoffible to afcribe to any other caufe, than to a power in the fluid to conduct caloric. Any other that might be imagined can be proved not to exift.

For the fides of the veffel could not conduct:

Thus it is evident, that the fides of the veffel could not convey to the fluid in contact with the bulb of the thermometer, any part of the caloric it received. Ice in common with

with any other ſolid may at temperatures below its melting point conduct caloric; but as it cannot poſſibly exiſt with a temperature above 32°, it cannot communicate any temperature above that to a fluid in contact with it, and conſequently it could not contribute in the above experiments to raiſe the thermometer above that temperature.

The experiment was performed in ſuch a manner that no motion was occaſioned in the fluid capable of conveying any part of it directly heated to the bulb of the thermometer; and indeed the regular, and in ſome of the experiments, the ſlow riſe of the thermometer is inconſiſtent with the ſuppoſition of caloric being communicated by any cauſe of this kind. neither was the fluid agitated;

It has been proved, that any current produced in the fluid by the contact of the heated ſolid could not be the mean of conveying caloric; for if the fluid be ſuppoſed to have no conducting power, ſince the ſides of the veſſel are likewiſe incapable of communicating any increaſe of temperature, the portion of fluid beneath the ſurface could not poſſibly be diſturbed by a heated ſolid merely reſting on that ſurface. Even if the ſolid were immerſed to ſome depth in the fluid, it is only from the ſides and under part of it that a current could riſe, and of courſe no part beneath that level could be affected. nor could any heated current deſcend;

It has likewiſe been ſhewn, that the riſe of the thermometer cannot be aſcribed to the contact of particles directly heated by the ſolid, and reaching the bulb from the ſinking of the column of fluid by the enlargement of the diameter of the veſſel from the melting of the ice; 1ſt. becauſe in the experiment with the oil this ſinking did not take place, the water produced ſupporting the oil at the ſame height; and 2dly. becauſe even in the experiments with the mercury, the dimimution did not take place to that extent which would have been neceſſary to have brought the particles which were heated on the ſurface of the fluid into contact with the thermometer, as was proved by actual meaſurement, and by the phenomena of the experiment itſelf. nor did the maſs of warmed fluid ſubſide to the thermometer;

Laſtly, the caloric which reached the bulb of the thermometer could not have been propagated by radiation, becauſe caloric, it has been proved, does not radiate through tranſparent fluids; and it cannot even be ſuppoſed capable of paſſing by radiation through an opaque fluid as mercury. The laſt point is obvious, and is of itſelf deciſive with reſpect to this objection. nor was the heat tranſmitted by radiation.

objection. But even the firſt, that caloric does not radiate through a tranſparent fluid, has been ſufficiently proved by the experiments of C. Rumford. I ſhall add one of a ſimilar kind made with the particular apparatus uſed in the preceding experiments, which appears to me ſufficiently deciſive. The thermometer in the ice cylinder was covered with oil one quarter of an inch, and the cup was ſuſpended over it in ſuch a manner that the bottom of it, though near to the ſurface of the oil, *did not touch* it. Boiling water was poured in, the thermometer roſe ſcarcely one degree in five minutes, and did not riſe more in any longer time. It has been ſhewn that when the thermometer was covered with one quarter of an inch of oil, and the cup filled with warm water, was ſuſpended ſo as to *touch* the ſurface, it roſe in ſeven minutes $5\frac{1}{2}$ degrees. Had this riſe been owing to the communication of caloric by radiation, the variation in the experiment which has juſt been related, could not have produced any change in the reſult. The communication of temperature by the *conducting power* of the fluid, muſt be facilitated by the contact of the fluid with the heated ſolid, but the communication of it by *radiation* required no ſuch aid, nor could it be promoted by it. The mere circumſtance of the heated ſolid not touching the fluid, could make no difference with reſpect to the one, but muſt make the moſt eſſential difference with regard to the other, and the experiment clearly proves, that even through tranſparent fluids caloric does not radiate. The ſlight riſe of temperature that took place, is owing to the caloric paſſing partly by *radiation*, and partly by communication by the medium of the air to the *ſurface* of the fluid, and from that it is conveyed by the conducting power of the fluid to the thermometer. The proof of this is, that even with mercury, a ſlight riſe of temperature was indicated when the experiment was performed in the ſame maanner. When the cup with hot water was ſuſpended over the mercury covering the bulb one quarter of an inch, but not touching it, the thermometer roſe one degree.

Experiments of radiation; with remarks.

Concluding remarks.

It may therefore from the whole of theſe experiments be concluded, that the obſerved riſe of temperature was owing to the fluid conducting the caloric, ſince no other cauſe can fairly be aſſigned. I have not ſubjected any other fluid to experiment, becauſe it was difficult to find any of a nature ſufficiently different, which the ice would have been capable of

of containing. But if it be proved that oil and mercury are capable of conducting caloric, it will be admitted as fufficiently probable that other fluids muft have a fimilar power. Of thefe two, mercury it is probable, from thefe experiments, is the beft conductor, as the rife of the thermometer took place in it much more rapidly than in the oil. That the temperature did not rife higher in it than in the oil, feems to have been owing partly to its greater mobility, by which its parts would move towards and from the ice with more celerity, and partly to its better conducting power, by which it would give out its caloric more quickly to the large fuperficies of ice with which it was furrounded. It was accordingly obferved, that the fides of the ice cylinder were more excavated in the experiments with the mercury than in thofe with the oil.

Edinburgh, Feb. 27, 1802.

II.

Defcription of a new Efcapement for Watches, invented by Mr. John De Lafons *.

The perfection of chronometers depends chiefly on the equality of the impulfes given to the balance.

THE inventor ftates, that fince the perfection of chronometers confifts more in the equality of impulfe given to the balance than to any other caufe, he has contrived the prefent efcapement for giving fuch an impulfe; with the additional qualities of locking the wheels without fpring work perfectly fafe, and unlocking them with lefs confumption of power than in any other efcapement he knows, becaufe the wheels do not bear againft the locking with more than one tenth part of the whole preffure from the main fpring, which laft circumftance he conceives to be perfectly new.

Efcapements of Mudge and Haley.

He remarks, that the equality of impulfe has been effected with great ingenuity by Mr. Mudge and Mr. Haley; but at the fame time he objects to the efcapement of the former, on account of its extreme difficulty and expence, and to that of the latter, for its very compound locking. And he alfo re-

* From the Tranfactions of the Society of Arts. 1801. A premium of thirty guineas was given by the Society to the inventor: I have not copied the letter prefs, as I wifhed to render the defcription rather more minute than was neceffary in that place.—N.

plies

plies to some observations which were made at the committee of the Society, which do not require to be stated here.

Description of a new escapement. Its parts.

Plate XIV. exhibits the escapement, where the same letters denote the same things in all the figures. A is the scape-wheel, B a pallet lever on an arbor with fine pivots, having at its lower end, Fig. 1 and 2, a remontoire, or spiral spring, C, fixed with a collar and stud as pendulum springs usually are. H H represents the balance, having upon its verge K K, a pallet D, consisting of two arms, which carry a small roller moveable on fine pivots; and also certain other pallets E, represented more fully in Fig. 4, the plan of which parts is seen in Fig. 1, at E and D. F is an arm which serves to move two locking pallets *a* and *b*. It is continued beyond the center of motion, in order that it may balance itself in all positions, and at the part most remote from the balance there are a couple of studs and screws to adjust and bank the quantity of motion. The last mentioned pallets have their faces, portions of circles, and they move on an arbor with fine pivots, as may be seen at Fig. 2 and 4, in which figure also it is to be observed, that the arm of the locking pallets is formed into a triple fork, the branches of which are exposed to the action of two pallets E before mentioned.

Performance of the escapement. 1. The scape wheel winds up a lever and spring; and is caught by a detent. But the re-action of the spring lever renders the hold of the detent exceedingly light. 2. The balance unlocks the wheel which lets go the lever, and this strikes a pallet on the balance arbor. 3. In its return the balance again unlocks the wheel, which again winds up the lever, (No. 1, 2, 3, &c.)

From the above description, and the consideration of the figures it will be apprehended, that, supposing the maintaining power to urge the wheel A forward, and the balance to be set in motion by external means, the lever pallet B will be wound up to its position in Fig. 1. by the tooth 1 of the wheel, at which instant the tooth 3 rests upon the pallet *a*. Suppose the balance to move in the direction of the small arrow near D, the verge pallet D will just pass the end of the lever pallet B, at which moment one of the two unlocking pallets E will carry before it the arm F, and unlock the pallet *a*, and consequently the wheel will drop with its tooth 4 upon *b*, and the lever pallet B will be set at liberty by the advance of the tooth 1, to act upon the balance by the pallet D, until B is stopped against the tooth 2, as seen in Fig. 5. By this means the balance receives the same invariable impulse at every second vibration. In its return the opposite unlocking pallet takes the other external arm of the fork of the locking pallets, and by that means unlocks *b*. The wheel immediately drops forward, almost through the space of one tooth,

tooth, ſo that the tooth 1, Fig. 5, falls upon the pallet *a*, while the tooth 5 winds the lever pallet B again into the poſition exhibited in Fig. 1. And the balance upon its return receives a ſecond impulſe under the circumſtances already detailed.

It remains only to be ſhewn in what manner the middle prong of the fork is employed. Its uſe is to ſecure the locking of the pallets *a* and *b*. When the unlocking is performed by the action of either of the pallets *m m*, Fig. 4, the middle prong falls into the ſmall notch in the roller *n*, which lies between thoſe pallets; and when the unlocking is performed, and either *a* or *b* has received its tooth, the arm F renders it impoſſible it ſhould be accidentally diſengaged, becauſe that arm is prevented from returning by the circular face of *n*, paſt which it could not return at all, if the notch or excavation were not made to permit to paſs at the proper time. The detents or locking pallets have no ſpring, and are very ſafe.

Laſtly, as the pallet lever B always reſts againſt one of the teeth of the wheel A, the preſſure againſt the pallets *a* or *b* may be rendered extremely light, ſo as to deduct ſcarcely any thing from the momentum of the balance at unlocking. Light preſſure on the detent.

Fig. 3. exhibits a ſimpler mode of conſtruction, in which a ſpring is ſubſtituted inſtead of the pallet lever with its ſpiral. The inventor remarks, that he ſhould prefer this conſtruction for a clock, but that the diſadvantage from the weight of the ſpring in different poſitions is obvious *. Another conſtruction.

III.

Remarks on the Proceſſes for clarifying Liquids. By CITIZEN PARMENTIER.

(*Continued from page* 234.)

NEVERTHELESS, though the uſe of the filter for water intended to be drank is of high antiquity, we muſt confeſs that the fountains conſtructed for this purpoſe not only deprive them of the earth which rendered them muddy, but likewiſe Filtered water is inſipid, becauſe deprived of gas.

* Some account of Time-pieces and Eſcapements are given in the Philoſophical Journal, Quarto Series, I. 56, 429. II. 50. III. 187, 342, 416.

of

of a super-abundance of air with which they are sometimes impregnated, and gives them that lightnefs or brifknefs, which is found in the waters of the Seine in a greater degree than in any other known ftream. The proof of this is, that by repeated filtrations we may render water flat, heavy and unwholefome.

Thus when the fpecific gravity of the water of the Seine is required to be determined, it is advifeable to take it from the river at the time when it is limpid, or leave it to clear by fubfidence, and not to take that in preference which has been filtered; for though this operation renders waters clearer, it changes them remarkably, by depriving them, as has been obferved, of the air which they contain in fuper-abundance.

I know a perfon whofe palate was fo accuftomed to water, that he could diftinguifh by the tafte water filtered through fand, from that which had not been fo filtered. The latter appeared to him more fapid and lighter; which proves, no doubt, the privation of this air, a fact which may alfo be very eafily afcertained under the receiver of the air-pump.

The filter cannot render falt water frefh.

Certain individuals, who are interefted to maintain the contrary to what is here afferted, have affirmed, that if water were continually forced to pafs through 18 feet of fand of gravel upwards, it would be purified not only of heterogeneous matters, but completely purified that is to fay of its falts. This prejudice was fuch, that in order to fupport the notion the following reafoning was offered:

If thefe filters be fufficient to deprive water of its air, why fhould not the fame operation be equally proper to deprive it of the falts with which it is charged; but no attention was paid to the circumftance that thefe falts held in folution in the water, being fpecifically heavier, pafs along with it through the fmalleft apertures, whereas air being fpecifically lighter, and exifting in a different ftate from that of the falts, is eafily feparated. And this procefs of application was propofed to be applied to fea water in order to render it potable. The operation has even been announced to the government as new and ingenious. It confifted in a filtration effected by force in a direction contrary to that of gravity.

But the union of faline matters with water has not a purely mechanical divifion. They are not interpofed as fome philofophers have pretended, but perfectly diffolved in water, and

3 poffefs

possess the same fluidity. These salts consequently become capable of passing through the finest filters. No other means but evaporation is therefore adopted to separate water from the saline substances which it holds in solution, and consequently distillation alone can produce this desirable effect. But to return to the general effects of filtration:

The filtering stone;

Independently of the filters here mentioned, water is also clarified by means of the stones called filtering stones. Of these there are several kinds. They are very porous, because sand enters into the greatest part of their composition. These stones being excavated are filled with water. The fluid gradually insinuates itself through their pores, and appears on the external surface in the form of drops of considerable clearness, which fall into a vessel placed beneath.

how managed.

It is necessary that these stones should be previously washed by filtration of a large quantity of water. It is observed, that even for several days the water acquires a disagreeable taste, owing to the foreign matters dissolved by this fluid in its passage through the stone. Water thus clarified cannot be fit for use until it passes through intirely deprived of taste.

It is a bad contrivance.

In general the filtering stone, though highly extolled, is a bad instrument to procure good water; for the filtration is made very slowly, and very often stops altogether, if the inner and exterior surfaces of the stone be not rubbed from time to time with a coarse brush, to detach the earth which the water deposits. To these inconveniences no doubt it is, that we ought principally to attribute the disuse into which this kind of filtration is fallen.

Clarifying without the filter.

It now only remains, that we should speak of the other processes which are used to give that perfect limpidity to several fluids, which they can never acquire by spontaneous clarification or by filters, whatever may be their structure.

Opacity from imperfect solution;

Though it is true, that the remarkable opacity of some fluids is owing to the interposition of particles not dissolved, but merely suspended in consequence of their extreme division; it is also certain, that under other circumstances the defect of transparency depends intirely upon the incomplete solubility of one or more bodies contained in these fluids, so that in order to give them the desired limpidity, we must necessarily have recourse to the methods which increase the solubility of the substances in question, or effect its total separation.

Albumen

removed by certain additions.

Albumen, gelatine, the acids, certain ſalts, lime, blood, and alcohol, may in many caſes concur to operate the clarification of certain fluids, for which the common filters would be inſufficient. Nevertheleſs, theſe agents cannot be indifferently uſed, and the preference given to one rather than the other always requires to be determined from a knowledge of the fluid to be clarified. Accident has ſhewn for example, that two handfuls of marle reduced to coarſe powder, and thrown into the fruit in the preſs, clarifies cyder and ſmall cyder.

Albumen and gelatine uſed in fining wines, &c.

The effect of the albumen and gelatine is principally ſeen in vinous liquors. For this reaſon alſo they are uſed in fining wine, that is to ſay, in producing that bright clearneſs which they can ſeldom acquire, and preſerve by ſimple repoſe. In this caſe it is ſufficient, that one or the other of theſe two ſubſtances ſhould be diſſolved in a ſmall quantity of water, and the ſolution mixed with the cold wine. A ſhort time afterwards a kind of net-work is ſeen to form itſelf through the whole fluid, and ſoon afterwards this net-work contracting in all directions, collects all the foreign ſubſtances, and carries them to the bottom.

In other inſtances it is neceſſary to heat the fluids with which the albumen has been mixed, and in theſe caſes the clarifying is effected at the inſtant the mixture begins to boil. Moſt ſyrups are clarified by this proceſs, and hitherto no other proceſs has been diſcovered to produce a better effect. It has alſo been obſerved that albumen alone is not always ſufficient to clarify liquids, even though they be heated ſufficient to cauſe them to boil; but that it is neceſſary to aſſiſt its action by means of an acid, or ſalt with exceſs of acid. In proof of this we may offer the clarification of whey as an example.

In ſome inſtances an acid is added.

In fact, it is proved that whey, in which albumen has been mixed, does not admit of the coagulation which carries the cheeſy matter along with it, unleſs a portion of acidulous tartarite of potaſh or vinegar be added at the inſtant the boiling begins.

It may alſo be conceived that the quantity of acid requiſite to be added in this caſe always bears relation to the ſtate of the fluid, and that it would be abſurd to pretend to fix the doſe in an invariable manner.

Freſh

Fresh cream is advantageously used to clarify spirituous liquors. One or two spoonfulls for each French pint or English quart are sufficient to produce this effect, without heat, in the course of a few hours. But as in this clarification there always remains some cheesy particles suspended in the fluid, on account of their great subtilty, it is necessary to complete the process by subsequent filtration. Cream used to clarify spirits.

Lastly, there are fluids which, in order to become clear, require only to be heated nearly to the boiling point. These are principally such as owe their opacity to substances which cannot be completely dissolved, unless the temperature of the solvent be raised considerably above the natural state. Many saline solutions are in this case, and those who are busied in chemical researches have frequent opportunities of observing them. Mere heat.

Most of the juices of plants newly expressed may be partly clarified by heat. The chemist is therefore in the habit of recurring to this method with respect to the juices which, on account of their density and viscidity, are not capable of being filtered. Clarifies the juices of plants, &c.

It frequently happens that a very slight degree of heat, applied to expressed and filtered juices of certain plants, will render them at once turbid; in which case the flocculent matter that floats in the liquid settles at the bottom of the vessel. This substance was considered by Rouelle the younger as the vegeto animal matter of wheat; but I shewed, in 1772, that it is a substance similar to the white of egg: which proves that we were authorized at that period to reckon the albumen among the products of the vegetable kingdom. Vegetable albumen precipitated.

I must insist on an important observation, that in general it is necessary to separate the magma which is formed in liquors clarified in albumen, particularly when in order to concentrate these fluids it is necessary to evaporate them by boiling. Without this precaution we shall see the same magma dissolve, and the fluids become more turbid than they were before the clarification. For a like reason it is that soups, which have not been skimmed in time, always retain a cloudy and unpleasant appearance. Caution again re-dissolution of the albumen.

Though the use of albumen to clarify the juices of certain vegetables is considerable, it is not, however, exempt from inconvenience. One of these inconveniences is, that it In what cases fining with albumen is hurtful.

changes the nature of thefe fluids fo much, that their medical properties are partly deftroyed. What happens to certain pharmaceutical preparations, fuch as decoctions of medicines, when white of egg and heat is ufed to clarify them, is well known; for, in that cafe, they become almoft ineffectual, if care has not been taken to double the proportion of ingredients which enter into their compofition. Lewis has obferved, that this operation deprives the fyrup of diacodium of all its medical properties.

Such are the obfervations which I have thrown together refpecting clarification. My defign in communicating them has been to prove that this operation, though fimple in appearance, cannot be practifed without fome fkill and attention; and that among the number of proceffes commonly ufed, feveral afford refults lefs fatisfactory than others. It is neceffary, therefore, to determine with regard to the choice of methods according to the nature of the fubftances made ufe of. I have confined myfelf to a clear narration of the facts; others may direct their attention to the explanations of the mode in which thefe fubftances operate.

IV.

On the Difcovery of the Arfeniates of Copper and of Iron. By a Correfpondent.

Difcovery of the arfeniates of copper and iron attributed to Vauquelin.

AN article * of the Decade Philofophique (30 Nivofe), giving an account of the firft trimeftre of the tenth year of the French Republic, fays, that Citizen Vauquelin has enriched the catalogue of mineral bodies by the difcovery of two new fubftances, arfeniate of copper and arfeniate of iron. I am far from imputing to this able analyft the defire of attributing to himfelf any difcovery to which he has no title; but the French chemifts, whether from ignorance or intention, pay fo little regard to the difcoveries of others, that it becomes the duty of every friend of fcience to point out their violations of hiftorical juftice.

True ftatement of their hiftory. Klaproth.

In the year 1786, Mr. Klaproth examined an ore from Cornwall, and found it to be arfeniate of copper. Schlotheion publifhed a defcription of it in 1790.

* By Cit. Lacepede, Secretary.

In

In the firft volume of the Annalès de Chimie, page 195, there is an imperfect fketch of an analyfis of arfeniate of iron by Mr. Prouft. Prouft.

In February, 1801, the Count de Bournon prefented to the Royal Society a very long and able account of a great number of varieties of thefe ores, which he had the good fortune to procure from Cornwall. This paper was followed by Mr. Chenevix's chemical analyfis, which proved the accuracy of the Count's defcription. The ample account of de Bournon, and Chenevix.

The moft extraordinary part of the paragraph in the French Journal is a confeffion, that the fpecimen examined by Cit. Vauquelin had been fent to the Ecole des Mines of Paris by the Count de Bournon. That gentleman never did fend any until his account and Mr. Chenevix's had been made public. Cit. Vauquelin's paper muft have been read at the National Inftitute between September 20 and December 20, 1801, fix months at leaft after the communication to the Royal Society. Hence we fee the juftice of attributing that difcovery to the French chemift. Extraordinary circumftances attending this *fuppofed* miftake.

In the Journal de Phyfique of Brumaire, tenth year, there is a paper of Dr. Karften, of Berlin, upon the fame fubject: but which deferves no notice, further than as it regards the hiftory of this fubftance. Dr. Karften's paper.

I am, SIR,

Your humble Servant,

A CORRESPONDENT.

V.

On the Nature and Preparation of drying Oils; with a View to the Improvement of fuch as are ufed by Artifts, as Vehicles for painting. By Mr.* Timothy Sheldrake.

EXPRESSED oils, confidered with a view to the painter's ufe of them, may be divided into two kinds; firft, fuch as are capable of drying in fome circumftances by themfelves, and always with certain additions; and fecondly, fuch as cannot be made to dry by any means whatever. Drying oils, and fuch as do not dry.

* Tranfactions of the Society for the Encouragement of Arts, for 1801, page 209.

Drying oils; linseed, nut, and poppy.

Of the firſt, which I ſhall call drying oils, there are three in common uſe, linſeed, nut, and poppy oil. The firſt is darkeſt coloured, and dries the ſooneſt; the ſecond is lighter, but does not dry ſo ſoon; and the third has leaſt colour, and dries ſlower than either of the others.

Drying oils contain mucilage.

By a proceſs, which it is perhaps needleſs to deſcribe here (*ſee Vol. xvii. page* 281),* I have ſucceeded in ſeparating from each of thoſe oils, a mucilage or gum, in a liquid ſtate, and capable of mixing with water in every proportion, though, when thoroughly dry, it would not diſſolve in cold water; but my experiments on this head were not carried to any great length. It is to be remarked that linſeed oil afforded moſt of this gum, nut oil the next largeſt quantity, and poppy oil the leaſt of all.

Olive oil contains none:

Olive oil, when treated in the ſame manner, afforded none of this mucilaginous ſubſtance; whence I was led to conclude that the eſſential difference between the drying oils, and thoſe which do not dry, conſiſts in this:—that the latter either contains no mucilage or gum, or that it is ſo intimately combined with its other principles, that it cannot be ſeparated from them in that peculiar manner which always takes place in oils which dry by themſelves, or when mixed with colours.

Drying oil affords a pellicle, which dries firſt, &c.

If drying oil is expoſed to the air, in a ſhallow veſſel, and left at reſt, a pellicle is ſoon formed on the top, and becomes externally perfectly dry. If this be removed, a ſecond will be formed in the ſame manner; and if this experiment be repeated many times on the ſame quantity of oil, without moving or ſhaking the veſſel, it will be found that the ſecond pellicle will require more time to form it than the firſt, and ſo on, till it will be found difficult to get it fairly ſkinned over in a conſiderable time. The ſame effect takes place, in a leſs viſible manner, in every quantity of drying oil which is united with colours in a picture.

From this experiment it is to be concluded, that drying oils exert that faculty by throwing up their mucilaginous parts, which become ſolid when at reſt, and in contact with the air.

* Or Philoſ. Journal, quarto, III. 345.

The

The ingredients added to oils to make them dry fafter, viz. calces of lead, faline fubftances, earths or gums, are fuch as unite with and increafe the quantity of thofe parts which float to the top, and form a fkin, more or lefs dark, over the colours originally mixed with them. If we confider the nature of thefe ingredients, we fhall be at once enabled to account for a fact univerfally known, viz. that in proportion to the ftrength of the drying oil ufed in painting a picture its colour becomes depraved. It will be injured and finally deftroyed, by being kept in a damp fituation, excluded from a free circulation of air, or placed under a glafs.

When oils are rendered ficcative they deprave the colours; and how.

The defideratum is to prepare oil or other vehicle for painting, fo that the colours, when mixed with it, fhall not be debafed under any of the above-mentioned circumftances. It muft be fo prepared or ufed, that it fhall ferve as a cement to unite and bind the colours, without fkinning over them. It muft likewife not contain thofe principles which always exift in the calces of lead, faline, or earthy fubftances, which from the firft deprave the colours, and attract particles from the air, under peculiar circumftances, which increafe that depravity, till at laft the appearance of the colours is totally deftroyed.

Oils for painting ought not to fkin; nor contain changeable ingredients.

It is only among the refins or bitumens that we can expect to find a fubftance poffeffing the properties requifite to give to colours all the brilliancy and durability of which they are fufceptible. My firft attempts and experiments were made with folutions of maftic and fandarac in the painters oils; but though thefe compofitions poffeffed more brilliancy than the common drying oils, they were liable to a confiderable objection; for they did not dry readily, and when dry, were eafily acted upon by all the common folvents for refinous fubftances, and on that account muft be very deficient in durability, which is one of the moft neceffary qualities I wifhed to difcover.

Refins or bitumens anfwer this character;

Maftic and fandarac dry flowly, and are not durable.

The difficulty with which amber is in any way diffolved, fuggefted the propriety of trying that fubftance. Accordingly I diffolved it, in each of the painters oils, by Dr. Lewis's procefs, without injuring its colour; and this folution was made in the common way. It was much darker coloured in itfelf, but produced fcarcely any difference in effect when mixed with colour. By experiments with each of thefe folutions I afcertained the following facts, viz.

Amber poffeffes fuperior qualities.

Every

Experiments. Tints.

Every colour, and all the tints compounded from it, were more brilliant than correfponding tints and colours mixed with the beft drying oils to be procured from the fhops.

Paintings fhut up;

Colours mixed with amber, after having been fhut up in a drawer for feveral years, loft nothing of their original brilliancy. The fame colours tempered with oils, and excluded from the air, were fo much altered, that they could fcarcely be recognized.

colours expofed to heat;

Colours tempered with amber were laid on plates of metal, and expofed (both in the air and clofe boxes) for a long time, to different degrees of heat, from that of the fun in fummer to the ftrong heat of a ftove, without being injured. It is needlefs to obferve that oil colours cannot undergo the fame trials without being deftroyed.

and to folvents;

Thefe colours, when perfectly dried in any way, were not acted upon by fpirit of wine and fpirit of turpentine united. They were wafhed with fpirit of fal ammoniac, and folutions of pot-afh, for a longer time than would deftroy common oil colours, without being injured.

They dry very well.

They dry as well in damp as in dry weather, and without any fkin upon the furface. They are not liable to crack, and are of a flinty hardnefs; whence it appears that this vehicle poffeffes every defirable property, and it is prefumed may be a difcovery of fome importance to artifts.

Gum copal nearly as good as amber, and rather brighter.

Having fucceeded thus far with amber, I tried the fame experiments upon folutions of gum copal, which is nearly as hard and infoluble as amber itfelf. The refult of thefe was the fame as the former, except that with the copal the colours were fomething brighter than with amber. As it is extremely troublefome to diffolve the copal and amber, I tried thofe folutions of them in oil which are fold in the fhops. When good, I found them to anfwer as well as my own. This is a great convenience, as many might be deterred by the difficulty of preparing this vehicle, who may willingly ufe it, as it is thus to be procured without that trouble.

The Method of ufing the Solution of Amber or Copal, as a Vehicle for painting.

Inftructions for the ufe of amber or copal as a vehicle for painting.

The cloth or other fubftance to be painted on, fhould be prepared with fome colour fully faturated with drying oil; or it will be better done with the fame vehicle to be ufed in painting.

painting. If it is not fully ſaturated, it will abſorb ſome of the vehicle from the colours, which is commonly termed the colours ſinking in.

All the colours which require grinding, ſhould be previouſly ground in ſpirit of turpentine. All the pure parts ſhould be tempered with ſuch a quantity of the vehicle as will enable them to lie on the pallet. The white ſhould be tempered as ſtiff as poſſible. All the tints ſhould be made by mixing the colours ſo prepared without any more of the vehicle, but they ſhould be diluted with ſpirit of turpentine, if neceſſary for working. Grinding colours, &c.

If the ground is properly prepared, and the above caution obſerved in tempering the colours, it will be found that all the dark colours in the picture will bear their full tone, and have a demi-tranſparency, which increaſes their native brilliancy, without the dingy appearance ſo common in ordinary oil-painting. The admixture of white increaſes the body of the colours progreſſively, till there will be left in the lighteſt parts, only ſo much of the vehicle as will bind the colours, and give them their full tone, but with very little of a ſhining appearance. When the picture is perfectly dry, it ſhould be varniſhed with a maſtic or ſimilar varniſh. Perhaps the beſt would be copal varniſh made by ſolution in ſpirit of turpentine, or ſpirit of wine. Conſequences or effects.

The rationale of this vehicle ſeems to be this: the amber and copal, when diſſolved in oil, form a homogeneous maſs, which dries by inſpiſſating, inſtead of ſkinning over, like the common drying oils, which conſiſt of heteregeneous parts, ſome of which ſeparate and dry on the top. Obſervations.

As the amber and copal are not ſoluble in any of the menſtruums which diſſolve moſt reſinous ſubſtances, pictures painted with them cannot be injured, if cleanſed with thoſe menſtruums: and as they are extremely hard, and the moſt durable ſubſtances of their claſs, they protect the colours from every kind of injury, more effectually than any other known vehicle.

No.

Conjectures to show that the foregoing vehicles were used by the older painters.

No. III. *Conjectures tending to show that the Vehicle which I have described, is similar in Principle, if not identically the same, as that used by several of the older Painters, who were eminent for their Skill in colouring.*

Lomazzo.

Lomazzo, an eminent painter, and pupil of Leonardo da Vinci, published a Treatise on Painting, in which it is mentioned that linseed or nut oil was generally used for painting: he likewise observes, that powdered glass was used as a dryer. As Lomazzo was blind when he published his treatise, he could have no motive for keeping any thing which he knew secret; whence it is to be concluded, that those oils were generally used for painting in his time, and that he knew of no exceptions to the practice.

Leonardo da Vinci.

In one part of L. da Vinci's Treatise on Painting, he mentions *nut-oil and amber*. As we know that amber gives peculiar brilliancy to colours, that L. da Vinci was peculiarly celebrated for the richness of his colouring, and are informed from his own writings that he was acquainted with solution of amber in nut-oil, it is to be presumed *that* was the vehicle he used. If this supposition is not to be admitted, we must believe that he knew how to dissolve amber in nut-oil (a process at that time both tedious and troublesome), without knowing the best use to which he could possibly apply it.

Leonardo's biographer says, "When he was at Rome, Leo X. resolved to employ him. Leonardo hereupon sets himself to the distilling of oils, and the preparing of varnishes to cover his paintings withal: of which the Pope being informed, said, pertly enough, that he could expect nothing of a man who thought of finishing his works before he had begun them. Leonardo therefore left Rome without having been employed."

I must beg leave to dissent from his Holiness's opinion. If my idea of Leonardo's vehicle be just, it was natural for him to begin the preparation of it as soon as he knew that he was to be employed as a painter: and as the spirit of that time led every one who made any useful discovery, to preserve it as a valuable secret, it was equally natural for him to account for his employment by saying that he was preparing varnishes. Whatever his secrets were, they remained unknown to the world till 1651, when his Treatise on Painting was published.

The

Robert Boyle.

The next intimation of folutions of amber which I have obtained is from the works of Boyle, who gained much of his information from Italian chemifts; whence it is evident that the knowledge of this preparation is of long ftanding in that country; and its ufe, if it was ufed at all in the arts, is to be fought for in the works of Italian artifts.

On the pictures of the Venetian fchool.

Whoever examines the Venetian pictures with attention, confiders that the beft artifts of that fchool were remarkable for the facility with which they worked, and reflects on fome paffages in Lomazzo, will be difpofed to admit that the peculiar fkill of the Venetian painters depended on three circumftances, viz. the colours they ufed, their method of ufing them, and the vehicle they worked with. Of the firft, Lomazzo gives pofitive information: the fecond can never be known without information equally pofitive; but of the vehicle fome knowledge may be obtained by way of analyfis. Till that knowledge is obtained, I may perhaps be excufed for hazarding the following conjectures.

Superior brilliancy of colour from the vehicle,

If my experiments have not mifled me, I am entitled to draw the following conclufions from them. Wherever a picture is found poffeffing evidently fuperior brilliancy of colour independent of what is produced by the painter's fkill in colouring, that brilliancy is derived from the admixture of fome refinous fubftance in the vehicle. If it does not yield on the application of fpirit of turpentine and fpirit of wine feparately or together, or to fuch alkalies as are known to diffolve oils in the fame time, it is to be prefumed that vehicle contains amber or copal, becaufe they are the only fubftances known to refift thofe menftrua.

which appears to have been amber or copal.

I have been told, and fome experiments of my own prove the information to be true, that the Venetian pictures, confidered with refpect to vehicle, are of two kinds: for fome are extremely hard, and not at all affected by any of the above menftrua; others are fimilar in colour, but fo tender that it is fcarcely poffible to clean them without injury, and in that refpect are little fuperior to mere turpentine colours. The firft, in confequence of the data which I have laid down, incur the fufpicion of being painted with amber or copal, but how are we to diftinguifh with which?

Attempt to make the diftinction between them.

As each of thefe fubftances refifts equally the common menftrua, perhaps the diftinction can only be made by afcertaining

taining the date of the picture. For example:—if it is found to have been painted before copal was known in commerce, it may safely be said to have amber for its basis; but if it has been painted after that period, I know of no method of distinguishing which of the two was made use of. As copal could not have been known, as an article of trade, before the seventeenth century, it follows that all pictures painted before that period, and possessing the properties I have described, must have amber for the basis of their vehicle. As this exception necessarily includes all the Venetian artists of the first class, we are therefore authorised to conclude that, if the works of these artists can bear the test of the menstrua I have mentioned, amber was the basis of the vehicle with which they were painted.

Ancient recipe, in which turpentine was the solvent of copal.

I once saw a recipe for dissolving copal, said to have been brought from Venice towards the close of the last century. The process was, to melt Venice turpentine upon the fire, to add gradually copal powdered, stirring them together to be united in fusion, and afterwards spirit of turpentine, in order to dilute it to the consistence of varnish. I tried this process, but it did not succeed.

It was not common, but Chio turpentine.

Upon inquiry I found that the Venice turpentine of the shops was only common resin, dissolved in spirit of turpentine to a proper consistence; whence the cause of my failure was evident. Reflecting on the commercial pursuits of the Venetians in the fifteenth and sixteenth centuries, I was led to conjecture that the substance called originally Venice turpentine was the product of some country intimately connected with them. Pursuing this idea, I procured, with much difficulty, some Chio turpentine, repeated my experiment, and succeeded completely. Besides the property of uniting easily with copal, it had others that excited my attention. Common resins, if exposed to fire, burn with extreme fierceness and rapidity; but when some of this was laid on the point of a knife, and held in the flame of a candle, it melted and dropped down before it began to burn. It emitted a peculiarly grateful smell; was of a most beautiful pale gold colour; was more brilliant than any turpentine I had ever seen; and when diluted to the consistence of varnish, perfectly resembled in colour, a solution of copal which I made in spirit of turpentine with camphor.

This turpentine differs from the common sort.

I showed

I ſhowed ſome of this to a gentleman who was converſant in ſuch ſubjects. He told me that, when at Venice, he frequently rubbed pictures violently with his handkerchief, to try if he could diſcover what they were painted with; and when ſo rubbed, they ſmelt exactly like what I then produced to him.

Additional probability that the Venetians uſed it.

As I had previouſly perfected what I thought to be a ſuperior vehicle, with which this could not vie in hardneſs and durability, I did not proſecute my experiments with this any farther; but as it unites rapidly with copal, and poſſeſſes all its viſible properties, I may be permitted to conjecture that it would have ſimilar effects when mixed with colours: and if there was any ſecond, inferior, and common vehicle, ſimilar in its viſible properties to the laſt, and ſo much within the reach of the moſt ordinary painters, as to give their works one common mark with thoſe of the firſt artiſts, it would be difficult to point out a ſubſtance more likely to afford it than this which muſt have been common in their own country, ſince its name is ſtill attached to ſubſtances of the ſame claſs throughout Europe, though its real properties are now but little known.

This laſt compound leſs hard and durable than the former.

If this was the baſis of the common Venetian vehicle, it might have been uſed with or without oil. If the latter, the works of the common Venetian painters muſt have been mere varniſh painting: if the former, it muſt have been compounded with the oil, according to the principles I have already explained. I am inclined towards the latter opinion, from having heard an obſervation attributed to Bombelli, a celebrated Venetian painter, who ſaid, "*That he wiſhed his pictures to dry as faſt as poſſible, that the oil in them might not riſe to the ſurface, and turn yellow.*"

It might be uſed with oil or without.

To this conjecture it may be objected that turpentines and compounds from them do not dry well. I am not prepared to anſwer this objection, as I have made no experiments relative to it; but it certainly is not concluſive, as ſuch compounds may not dry well in this country, though they may in the warm climate of Italy.

In the *Maniere d'imprimer les Tableaux*, publiſhed by Le Blond at Paris, 1740, is a recipe for the varniſh he uſed on the coloured prints executed by him, in this country, before he

Varniſh of Le Blond.

he went to France. It is as follows:—"Take four parts of balſam of capivi and one of copal. Powder and ſift the copal; and throw it by degrees into the balſam of capivi, ſtirring it well each time it is put in: I ſay each time; for the powdered copal muſt be put in by degrees, day after day, in at leaſt, fifteen different parts. The veſſel muſt be cloſe ſtopped, and expoſed to the heat of the ſun, or a ſimilar degree of heat, during the whole time; and when the whole is reduced uniformly to the conſiſtence of honey, add a quantity of warm turpentine; *Chio turpentine is the beſt.*"

Its durability and excellence.

Le Blond's prints were long neglected, and are now forgotten. Whatever difference of opinion may prevail reſpecting them, there can be none reſpecting his varniſh, as I have ſeen ſome of theſe prints in perfect condition, notwithſtanding they had been thrown careleſsly about for nearly ſixty years.

His intelligence probably from Italy.

Le Blond was a pupil of Carlo Maratti. He died at a very advanced age, leaving behind him the character of an ingenious projector. It is probable that he might collect much information analogous to his purſuits during a long life; but it is more probable that he obtained much of it where he received his education. Thus, wherever we find notices of the uſe of theſe ſubſtances in the arts, they invariably lead us towards Italy, where they certainly were firſt known.

I have thus detailed the circumſtances which impreſs me with a conviction that the vehicle I have offered to public notice is, in ſubſtance, the ſame as that uſed by the beſt colouriſts of the Italian ſchools. What impreſſion the facts I have enumerated may make upon others I know not: but ſtill the truth of my opinion muſt be determined by experience; for it would be of ſmall conſequence to prove that this vehicle was uſed in former times, unleſs it can likewiſe be made evident that it will be uſeful to the preſent race of artiſts.

Yours, &c.

TIMOTHY SHELDRAKE,

Strand, February, 1801.

VI.—*On*

VI.

On the Native and Artificial Sulphurets of Iron, by PROFESSOR PROUST *.

IN a former memoir † I remarked, that the reafon why the acids which eafily diffolve the artificial fulphuret, do not exercife the fame power on the native fulphuret, confifts in an excefs of fulphur in the latter, which art has not yet fucceeded in combining with iron. Hitherto in fact, I have not prefumed that our imperfect means could approach fufficiently near thofe of nature for us to hope to imitate the formation of pyrites. Accident, however, has lately removed this difficulty.

Why native fulphurets of iron are not foluble in acids like the artificial.

I heated, without any particular attention to the quantities, a mixture of about ten ounces of fulphur and of filings, in order to fupply my laboratory; and judging by the colour that the quantity of fulphur was infufficient, I thought proper to add a new dofe. In confequence of this, the crucible was made nearly red hot; but not to fufion, becaufe it is more convenient for ufe to obtain it in the ftate of powder. When I afterwards tried to diffolve it in an acid properly diluted, I was fomewhat furprized to find that it did not afford fulphurated hidrogen. It was to no purpofe that I altered the ftrength of my acid, for I obtained no gas. This unexpected refult fhewed the poffibility of forming pyrites by art.

Iron pyrites formed by art.

Since pyrites becomes foluble as we have feen, only by depriving it of the fulphur which exceeds its point of faturation, it appeared to me, that I fhould firft endeavour to reftore all its qualities by producing its firft ftate from a fimilar excefs of fulphur; and this in fact fucceeded.

Roafted pyrites reftored by giving it the requifite dofe of fulphur.

I mixed an indeterminate quantity of fulphur with four hundred grains of the pyrites of Soria, deprived of its excefs by diftillation, and I heated the mixture in a retort befide another, which alfo contained four hundred grains of pyrites in the crude ftate. My object in this laft arrangement was to obtain a kind of thermometer, to prevent my giving too great a degree of heat to the firft retort; that is to fay, not to ex-

Experiments.

* Journal de Phyfique, Pluviofe X year.

† Philof. Journal, I. 109.

pofe

pofe this fecond dofe of fulphur, which a new attraction was to add, to that temperature which would conftitute the firft point of faturation with regard to iron.

At a given heat the fuper-abundance of fulphur was carried off by diftillation, after which the two retorts being kept an hour longer at the fame temperature, did not exhibit the flighteft vapour of fulphur.

Regenerated pyrites. The regenerated pyrites was pulverulent; a proof that it had retained no portion of fulphur beyond the point of faturation, otherwife I fhould have found it in a pafty ftate, having the figure of the retort.

Characters. It had refumed the greenifh yellow colour, which is that of crude pyrites when pounded, whereas its tinge was before dull and blackifh like fulphurated iron, which is capable of affording hidrogen. Its weight was found to be five hundred and four grains, that is to fay, the diftilled pyrites had refumed twenty-fix pounds of fulphur per quintal.

Proportions of parts.

Obfervations on the quantities; to prove that the artificial pyrites is the fame as the native. By infpecting the former memoir, we fee that the mean product of two diftillations, each confifting of four hundred grains of pyrites, was three hundred and eighteen for the refidue, and feventy-eight for the fulphur, to which muft be added three or four for what was carried away by the gas, fo that in the quintal we have feventy-nine and a half refidue, and twenty and a half fulphur. According to this report, four hundred grains of the refidue, or diftilled pyrites, ought to have taken only ninety-eight and a fraction of fulphur, whereas the refult of our experiment gives one hundred and four. The difference being no more than one and half per quintal, may be afcribed to the inaccuracy of experiment, as well as the nature of the pyrites, which is not an homogeneous combination; for befides the clay and the fand, it often contains a fmall portion of oxide of iron, which may have caufed the fixation of a fomewhat greater quantity of fulphur than the lofs it fuffered by diftillation.

Trial of the regenerated pyrites by acids; which did not diffolve it. I afterwards examined my regenerated pyrites with a fulphuric acid of ten degrees by the areometer of Baumé, which acid diffolves the diftilled pyrites very well; but I obtained only a few ounces of fulphurated hidrogen. I afterwards heated the mixture; it afforded a flight portion of gas, after which the pyrites remained without alteration, Long continued boiling was ineffectual for faturating my acid.

With

With the muriatic acid I ſucceeded in forming a ſmall portion of gas; but its action ſoon ceaſed, and it was equally impoſſible for me to ſaturate the acid. It was of twelve degrees of the areometer, and the powder preſerved its colour. The natural pyrites treated in the ſame manner does not afford the ſlighteſt ſuſpicion of gas; but we muſt not overlook that art cannot give to its compounds that aggregate condenſation which is one of the greateſt obſtacles to ſolution. Morveau and Fourcroy have given us very ſtriking proofs in the reſiſtance they found in diſſolving the native oxides of iron and tin.

The aggregation of native pyrites in ſome meaſure defends it.

The pyrites not being, as I have obſerved, an homogeneous combination, it is evident that we could not expect to diſcover the true proportions of ſulphur wherein iron could attract by theſe previous experiments; for which reaſon I made the following.

Theſe experiments could not ſhew the proportions of ſulphur.

In order to depend in the firſt place upon pure filings, we muſt begin by heating them gently, and for a conſiderable time in a glaſs retort. Under theſe circumſtances, it is not a little remarkable to obſerve, that filings cleaned with the magnet, and kept in well-cloſed bottles, afford nevertheleſs a very ammoniacal water, and even muriate, if I am not deceived in the taſte of the fluid which eſcapes.

Other experiments.

Iron filings afford ammonia.

One hundred parts of filings heated to a low red heat in a retort, and ſulphur being let fall upon them, acquire an incandeſcence which was noticed even by the ancient chemiſts; but they do not become ſaturated. The increaſe is found to be only twenty or thirty parts. After pounding the product, then mixing it with ſulphur, and expoſing it to a red heat, a reſult is obtained, which weighs pretty conſtantly 159; but which, I think, may be ſet at 160, on account of the impurities of the iron.

Direct combination of iron and ſulphur.

Firſt degree of ſulphurization. 60 parts of ſulphur to 100 iron.

The product is iron ſulphurated to the firſt degree. It may be fuſed, and even melts in the retort, if harpſichord wire be uſed, and maintains itſelf without alteration. Its colour is metallic, but dull and conſiderably different from the golden colour of the pyrites. In a word, it is the ſulphuret which is proper to afford ſulphurated hidrogen gas.

Its appearance.

In order to diſcover the exceſs which this ſulphuret can ſtill abſorb, provided its temperature be leſs elevated, I treated two hundred grains of filings with the before-men-

Second degree, 89 ſulphur to 100 iron.

4 tioned

tioned precautions, and obtained three hundred and eighteen grains of fulphuret. When taken out of the retort it was mixed with a new dofe of fulphur, and heated befide another which contained crude pyrites. The fuper-abundant fulphur paffed in diftillation, and the retorts were kept for an hour at the fame heat. The refult of this operation was an artificial pyrites, weighing three hundred and feventy-eight grains. It was pulverulent, which proved that it had retained no more fulphur than its attraction could take up.

Its appearance and habitudes.

Its colour was no longer blackifh, but greenifh yellow. When treated with the acids, its habitudes were precifely the fame as thofe of the regenerated pyrites; and laftly, it differed from the native pyrites only by the want of that denfity which the latter received from their humid cryftallization.

Confequences.

Recapitulation of the facts, with obfervations.

From the foregoing facts it follows, that iron can fix fixty per cent. of fulphur by a confiderably elevated temperature. This proportion conftitutes iron fulphurated to the minimum.

Sulphuret at the minimum 10 fulph. + 6 iron.

Sulphuret at the maximum 10 fulph. + 9 iron.

By a lower heat it can alfo attract a quantity which is equal to the half of this weight; and this refult is iron fulphurated to the maximum, or with ninety parts of fulphur. If this laft combination be expofed to the temperature which formed the firft, it returns to that ftate; that is to fay, it returns to the minimum of fulphuration, by giving out all the fulphur it was capable of fixing above the proportion of fixty parts for each quintal of iron. Iron fulphurated to the maximum is the pyrites, and poffeffes all its properties excepting denfity.

Pyrites reduced to the minimum by heat with iron filings,

In order to render the highly-fulphurated iron fit for affording hidrogen, it need only be heated with half its weight of filings.

or by diftillation,

To apply pyrites to the fame ufe, the fame treatment may be adopted, or they may be diftilled, in order to deprive them of that portion of fulphur which conftitutes the difference between the fulphuret at the minimum, and that at the maximum.

not found native.

The mineral kingdom has not yet prefented iron fulphurated to the minimum. In the yellow coppery pyrites, which is a compound of two fulphurets, the iron is always at the maximum of its faturation, and accordingly thefe ores refift all acids, except fuch as can oxide the excefs of fulphur.

Copper in both ftates?

I fhewed in my former memoir, that the native fulphuret of copper is ufually found with an excefs of fulphur amounting

ing to 14 or 15 per cent; it is very poffible, that this metal may in its faturation follow the fame law as iron, which requires to be examined; but only in the fulphurets which are pure; for in thofe which are complicated, or thofe in which the fulphuret of copper is an integrant part, I have found it without excefs of fulphur.

The yellow copper ores, or the native union of the two fulphurets of copper and iron fulphurated to the maximum, afford by diftillation lefs fulphur than the fimple pyrites, becaufe the fulphuret of copper in this mixture has no excefs. The beautiful copper pyrites of Avalar in Bifcay, affords no more than one twelfth of fulphur by diftillation. Copper pyrites, are mixed: their habitudes.

If thefe pyrites be fufed with a portion of potafh, the excefs of fulphur unites to this alkali, and the fulphuret of iron is reduced to the minimum. The aqueous fulphuric acid may then be ufed to analyfe them. It diffolves all the fulphuret of iron without affecting that of copper, which in that cafe prefents itfelf with a deep blue colour, which is one of its characters. By this means we may obtain the proportion of the two fulphurets which compofe this kind of mineral. But when we know the quantity of copper contained in a copper pyrites, we always know that of the fulphuret of the metal, becaufe in nature, as well as in art, copper never takes up more or lefs than 28 grains of fulphur per quintal. Analyfis of the compound pyrites.

If we wifh to afcertain the quantity of fulphuret in a copper pyrites, its nitric folution muft be precipitated by fulphurated hidrogen, and the precipitate ignited in a retort. The product always reprefents the real quantity of fulphuret originally combined in the mineral. Copper in nature and alfo by art takes up exactly 28 per cent. of fulphur.

Native Sulphuret of Manganefe.

This fulphuret has not been noticed by mineralogifts as far as I know. I difcovered it fome time ago in certain fpecimens of gold-ore from Nagyag. Native fulphuret of manganefe.

The gangue of the piece which afforded this fulphuret is a carbonate of manganefe mixed with quartz, like that of the fulphuret of Tellurium. It does not prefent metallic cryftals of this metal, but a multitude of points, which under the magnifier appear to be an affemblage of pyritous parts. This mineral treated with the aqueous fulphuric acid, affords a History and defcription. Treatment.

 very

very abundant mixture of carbonic gas and ſulphureous hidrogen. The firſt ariſes from the decompoſition of the carbonate, and the ſecond from that of the ſulphuret.

This new ſulphuret of mangaueſe very eaſily known.

It is extremely eaſy to aſcertain the exiſtence of this new ſulphuret. Firſt, becauſe among all thoſe we are acquainted with there is none which is ſo eaſily decompoſed, and with ſuch abundance of this gas. Secondly, becauſe the artificial ſulphuret of mangaueſe obeys the action of this acid with the ſame celerity. And thirdly, becauſe we do not diſcover any thing elſe in the ſolution but oxide of mangaueſe, and a very minute quantity of that of iron. The ſpecimen does not contain either gold, tellurium, lead, or any other metal.

Ore of tellurium.

In the true ore of tellurium, I found that the ſulphurets of lead and of tellurium are combined together, and that the gold is native, and not at all mineralized.

Is the mangaueſe, metallic, or oxided?

I ſhall add nothing further reſpecting the ſpecies of ſulphuret here mentioned, becauſe I have not enough to decide whether the mangaueſe exiſts in the ſtate of oxide or metal. If it be an oxide, its ſulphuret may probably derive from a very condenſed aggregation the power of eluding the activity of the atmoſpheric oxigen; for the artificial ſulphuret paſſes with conſiderable ſpeed to the ſtate of black oxide mixed with ſulphate.

Concerning the Diſ-oxidation of Iron.

On diſoxidation.

When we conſider the facility, or low temperature which ſeveral oxides require to return to the metallic ſtate, we cannot help thinking, that thoſe which reſiſt the effort of our furnaces, would alſo become diſoxided, if it were poſſible for us to heat them to the neceſſary degree.

Incident reſpecting the reduction of iron by mere heat.

In order that iron may no longer remain in the claſs of oxides which are incapable of reduction without the aſſiſtance of charcoal, I think it proper to record in this place an experiment, for which I am indebted to the friendſhip of Naſt, manufacturer of porcelain, at that time eſtabliſhed in the Fauxbourg St. Antoine.

A bar of iron oxided nearly all through was reduced by heat in a porcelaine furnace.

The mouth of his furnace for ſecond firing, which is above the furnace itſelf, was ſupported by a bar of iron about an inch in diameter. The oxidation had approached ſo near its centre, that being no longer ſupported but by a thread of iron of

of one line in diameter, it fell and broke to pieces. I collected the fragments, and separated the metallic thread by a slight blow. The oxide differed in no respect from the scales, which are separated from iron when forged.

I placed eight ounces of this oxide in a porcelain crucible inclosed in one of the saggers below. I was not ignorant that nothing more is necessary, than the fall of a single nail to destroy a whole pile. The following was the result: we found the crucible and the sagger perforated; the iron reduced and well fused had bedded itself in the floor of the furnace. We could not separate it but by blows of the chissel. It was not brittle. Did this iron melt merely by the intensity of the fire, or did it oxigenate some charcoal, or decompose the calcareous part of the pottery, &c. It would I think be an interesting object to ascertain this. I kept this iron for a long time, and do not at present know what is become of it. I was not sufficiently aware at the time how much the theory of chemistry might be interested in its examination. It is much to be desired that the experiment should be repeated.

Observations and queries.

Concerning Wax.

I think I may announce to you, that wax exists in the green fecula; I had before found it in that of opium, and expect to find it ready formed in the fecundating powder. I shall at some future opportunity treat it in the same manner as the fecula.

Wax in the green fecula of vegetables.

VII.

General Observations on the Causes which influence the Weather in England, and the popular Methods of judging of the Weather. By James Capper, *Esq. formerly Colonel and Comptroller General of the Army and Fortification Accounts on the Coast of Coromandel *.*

THOSE who are furnished with proper instruments, and who carefully observe the information they afford, will not often be mistaken in their judgment of the changes of the weather.

Philosophical instruments are of great utility for judging of the weather;

* From his "Observations on the Winds and Monsoons" 4to, London, 1801.

weather. The barometer, the thermometer, the hygrometer, and the electrometer, will generally give us timely notice of any material changes in the state of the atmosphere. But before we consider the best, or at least the usual modes of employing these instruments, we will beg leave to mention some common remarks of the peasantry, whose professions requiring them to live much in the open air, their opinions merit very great attention, being the result of local observation, continued from father to son, and verified from the experience of many ages. Amongst the first of these is one, now established into a proverb, that a rainbow in the morning is the shepherd's warning, but a rainbow at night is the shepherd's delight.

yet the common remarks of peasants are valuable.

Morning rainbow denotes rain; evening rainbow, fair weather.

In a country with the sea or ocean to the westward, and the wind from the same quarter, this opinion is likely to be true. If therefore, the clouds to the westward in the morning are saturated with moisture, which they must be to produce a rainbow, as these clouds proceed from the W. towards the E. they probably will produce rain; whereas, on the contrary, when the sun sets perfectly clear, and the clouds to the eastward are moist, it is a proof that the wet clouds are past, with a westerly wind, and the shepherd therefore may reasonably expect fine weather on the following day.

Explanation of the prognostic. With a west wind the morning rainbow to the west denotes coming showers; evening rainbow showers past.

When it rains with an east wind, it probably will rain for twenty-four hours. This is another observation, which seems to me applicable to countries situated as above-mentioned, with land to the eastward; for, in general, the weather is dry in these countries with an east wind, but when the cohesion of the air and water is broken, the rain will not be violent, but of long duration.

Rain, if from the east, lasts 24 hours.

Explanation.

The weather generally clears at noon, but when it rains at mid-day, it seldom clears up again till sun-set. The air, when dry and warm, continues to absorb and retain the moisture continually evaporated from the earth; as therefore the sun advances towards the meridian, and for an hour or two afterwards, he dries and warms the air; and consequently the rain is likely to cease at that time. But if there should be so much water in solution in the atmosphere, that the heat of the sun is not sufficient to produce these effects, in that case the rain will probably continue some hours longer.

Weather usually clears at noon, but if not, then at sun-set.

Explanation.

Violent

Violent winds generally abate towards fun-fet. If we admit that wind is only a current of air put in motion by the rarefaction of the atmofphere in fome particular place, and that this current of air is moving towards the point of rarefaction to reftore the equilibrium, we muft fuppofe, that as the fun declines the rarefaction will diminifh, and confequently the velocity of the wind decreafe. But this obfervation, in my opinion, rather applies to the temperate than to the torrid zone; for in whirlwinds and hurricanes the contrary may very often occur.

Strong winds abate at fun-fet because the change of temperature changes the caufe.

When the wind follows the courfe of the fun, it is generally attended with fair weather. This frequent and regular change of wind, which is never more than a moderate breeze, proves that there is no point of confiderable rarefaction near, and therefore, the current of air follows immediately the fun's courfe: it always happens in fummer, but very feldom when the fun's meridian altitude is lefs than 40 degrees.

Weather fteady when the wind follows the fun.

The changes which take place in the atmofphere are principally marked by the rifing and falling of the barometer, which apparently is caufed by heat and cold, the hands with which nature performs her meteorological operations: by the former the atmofphere is rarefied, and confequently becomes light; by the latter it is condenfed, and confequently becomes heavy. Hence probably the old remark, that a ftorm generally follows a calm; for during a calm the air is rarefied and expanded, and the cold air will rufh forward in a ftrong current to reftore the equilibrium, and neceffarily produce what is generally called a gale of wind, the violence of which alfo will of courfe be in proportion to the degree of its preceding rarefaction.

A ftorm follows a calm; becaufe the air over any fpot is more heated when quiefcent, and the furrounding air rufhes in.

For thefe reafons, the barometer falls fuddenly whilft the air is expanded before a gale of wind, and rifes again gradually as the condenfed air returns, and the gale in like manner by degrees fubfides.

The barometer confirms the fact.

It muft however be obferved, that an extraordinary fall of the mercury will fometimes take place in fummer, previous to heavy fhowers of rain, particularly if attended with thunder and lightning; but in fpring, autumn, and winter, the fudden extraordinary defcent of the barometer indicates principally violent wind.

Summer fhowers denoted by a fall of the barometer.

Upon

Why the barometer varies little near the equator, and very much in high latitudes.
St. Helena.
Madras.
S. of Europe.
England.
Peterſburgh.

Upon theſe principles likewiſe we may account for the riſe and fall of the barometer in the different zones. In the torrid zone, particularly at St. Helena, and the iſlands of the Pacific ocean, it ſeldom varies more than three tenths; at Madras about five-tenths; in the ſouth of Europe not more than one inch and two-tenths; in England it varies two inches and a half, and in Peterſburgh three inches four-tenths. In the two firſt, the temperature of the atmoſphere is not ſubject to much variation, and never to any great degree of condenſation. In the third, reckoning from the tropics to the latitude of 40°, the atmoſphere may ſometimes be ſuddenly condenſed by currents of cold air from the N. and ſtill more ſo in England. But the greateſt variation muſt neceſſarily take place on the continent to the northward, where, during the ſummer, the weather is as hot as within the tropics; and, in winter, the thermometer for many weeks, continues ſeveral degrees below the freezing point.

Thermometer; hygrometer; electrometer.

The thermometer alſo, which meaſures the degree of heat in the air near the earth, will contribute towards denoting when changes are likely to take place in the lower regions of the atmoſphere; the hygrometer diſtinguiſhes the quantity of moiſture in the atmoſphere, and the electrometer will point out the quantity of electricity which prevails in it.

The barometer little eſteemed by farmers, becauſe the words engraved thereon are fallacious.

The words generally engraven on the plates of the barometer rather ſerve to miſlead than to inform; for the changes of the weather depend rather on the riſing and falling of the mercury, than on its ſtanding at any particular height When the mercury is as high as fair, or at 30 degrees, and the ſurface of it is concave, beginning to deſcend, it very often rains; and on the contrary, when even the mercury is at 29 degrees, oppoſite to rain, when the ſurface of it is convex, beginning to riſe, fair weather may be expected. Theſe circumſtances not being known, or not duly attended to, is the principal cauſe that farmers and others have not a proper confidence in this inſtrument.

Daily change in the barometer, and why.

It muſt alſo be obſerved, that *cæteris paribus*, the mercury is higher in cold than in warm weather, and commonly early in the morning, or late in the evening, than at noon, which ſeems occaſioned by the obvious cauſes of the atmoſphere being condenſed by the cold of the night, and rarefied by the heat of the day.

The

The following obfervations of Mr. Patrick feem confirmed by experience. The old obfervations of Patrick are good.

1. The rifing of the mercury prefages, in general, fair weather, and its falling foul weather, as rain, fnow, high winds, and ftorms.

2. In very hot weather the fall of the mercury indicates thunder.

3. In winter the rifing prefages froft; and in frofty weather, if the mercury falls three or four divifions, there will certainly follow a thaw; but in a continued froft, if the mercury rifes, it will certainly fnow.

4. When foul weather happens foon after the falling of the mercury, expect but little of it, and, on the contrary, expect but little fair weather, when it proves fair fhortly after the mercury has rifen.

5. In foul weather, when the mercury rifes much and high, and fo continues for two or three days before the foul weather is quite over, then expect a continuance of fair weather to follow.

6. In fair weather when the mercury falls much and low, and thus continues for two or three days before the rain comes, then expect a great deal of wet, and probably high winds.

7. The unfettled motion of the mercury denotes uncertain and changeable weather.

But to thefe remarks may be added, that, when the barometer fuddenly falls two or three tenths, without any material alteration in the thermometer, and the hygrometer is not much turned towards moift, a violent gale of wind may be expected. When the hygrometer inclines far towards moift, with only a trifling defcent in the barometer, it denotes a paffing fhower and little wind; and when the barometer falls confiderably, and the hygrometer turns much towards moift, the thermometer remaining ftationary, and rather inclined to rife than fall, both violent wind and rain are likely to follow in the courfe of a few hours. Prognoftics of violent wind; of fhowers; or of a ftrong gale of wind and heavy rain.

General or common Prognoftics of the Weather.

Common prognoftics.

Amongft thefe we may reckon fuch as are derived from birds, beafts, infects, reptiles and plants, to which might be added great part of the wood work in houfes, as doors, windows, window fhutters, &c.

Birds

Birds drefs their feathers againft rain,

Birds in general retain in the quill part of their feathers a quantity of oil, which when they feel an extraordinary degree of moifture in the atmofphere, they exprefs, by means of their bills, and diftribute it over their feathers, to fecure their bodies againft the effects of an approaching fhower.

and fwallows fly low.

Swallows in purfuit of the flies and infects, on which they prey, keep near the earth in wet weather; and in dry weather, from the fame caufe, they fly much higher.

Cattle feed haftily, and go to fhelter.

Domeftic animals, as cows and fheep, but particularly the latter, on the approach of rain, feed with great avidity in the open field, and retire near the trees and hedges as foon as they are fatisfied. In fine weather they graze and lounge about, eating and refting alternately with apparent indifference.

The pimpernel and dandelion much affected by the approach of wet weather.

The pimpernel, commonly called peep-a-day, or fhepherd's weather-glafs, clofes its leaves before rain; and the down of the dandelion is much affected by moifture.

Wood work fwells.

All wood, even the hardeft and moft folid, fwells in moift weather. The vapours infinuate themfelves into the pores of trees, and alfo into the wood-work of houfes.

Infects alter their conduct in various ways.

Infects and reptiles of all kinds feek or avoid rain according to their refpective habits, by thefe means giving notice of every change of weather.

The drains emit a fmell;

because the air does not abforb the vapors.

On this fubject fee Philos. Journal, quarto, IV. 135.

It is a well known fact, that before rain, particularly in fummer, a ftrong fmell is perceived from drains and common fewers, as well as from every other body emitting a great quantity of effluvia. During fair weather, even in the fummer, the atmofphere readily abforbs all the vapours and exhalations from the earth until it is completely faturated, and confequently the effluvia from the bodies which emit them, will then be confined and afcend in a narrow compafs, like the fmoke of a chimney in dry weather, almoft perpendicularly; but when the air is faturated with moifture, and becomes rarefied and expanded, as it always does before rain, the volume of air containing the effluvia will be extended horizontally, and diverge from thefe different bodies as from a centre, and will be fenfibly perceived on all fides, but will of courfe be moft perceptible on that to which the current of air or wind moves.

Vapors feen in winter over water.

In winter, when the thermometer is between 34 and 40 degrees, the air being in a ftate of condenfation, and the running

running water being warmer than the land, a mift or fog may be feen rifing above the rivers, particularly when the air is cold and clear; but this vapour is no longer vifible when the river is frozen, for though the ice be fubject to evaporation, it does not yield fo much vapour as water, and the water, in parting with its caloric in the moment of freezing, warms the furrounding air.

To the philofopher all objects in nature, both animate and inanimate, may afford both amufement and inftruction, particularly in meteorology; but to obferve them with due attention, we muft quit the bufy fcenes of life; "and thus our "lives exempt from public haunts, find tongues in trees, "books in running brooks, fermons in ftones, and good in "every thing." Amufement and inftruction from the folitary contemplation of nature.

VIII.

Defcription of a Cheap, Simple, and Portable Inftrument, for determining the Pofitions of Objects in taking a Picture from the Life. By R. L. EDGEWORTH, *Efq.*

THAT active and intelligent philofopher and journalift, Cit. Pictet, author of the Bibliotheque Brittanique, on his late return from London to Paris, prefented various inftruments brought from this country to the National Inftitute of France. Among them was the inftrument here to be defcribed, and fince publifhed in the Bulletin des Sciences; on infpection of which my attention was excited to my notes, where, among other communications to be made to my readers, I find this inftrument, as publifhed in the excellent work "On Practical Education" by Maria Edgeworth and her father, to whom I have afcribed the contrivance in the title. The authors of the Bulletin affirm, that it was invented and executed by the children under the parental care of Mifs Edgeworth; but I find no fuch intimation in the original work. The inftrument of Profeffor Pictet is in various refpects inferior to that which I have here copied; infomuch that Cit. Cloquet has propofed an amendment, for giving the index an horizontal pofition when it is required to transfer the obfervation to paper, which is in fact lefs effectual than the provifions made for that purpofe, Inftrument for perfpective prefented to the National Inftitute; was not as perfect as the original invention.

pose and for fixing the true instrument. After this preface, I shall proceed to copy without farther remark, page 460.

Advantages of early habits of delineation, and the inspection of drawings, &c. of machines.

"An early use of a rule and pencil, and easy access to prints of machines, of architecture, and of implements of trades, are of obvious use in this part of education (mechanics). The machines published by the Society of Arts in London, the prints in Desaguliers, Emerson, le Spectacle de la Nature, Machines approuvées par l'Académie, Chambers's Dictionary, Berthoud sur l'Horlogerie, Dictionaire des Arts et des Metiers, may, in succession, be put into the hands of children. The most simple should be first selected, and the pupils should be accustomed to attend minutely to one print before another is given to them. A proper person should carefully point out and explain to them the first prints that they examine; they may afterwards be left to themselves.

Method of conveying the requisite previous information,

"To understand prints of machines, a previous knowledge of what is meant by an elevation, a profile, a section, a perspective view, and a (*vue d'oiseaü*) bird's eye view, is necessary. To obtain distinct ideas of sections, a few models of common furniture, as chests of drawers, bellows, grates, &c. may be provided, and cut asunder in different directions. Children easily comprehend this part of drawing, and its uses, which may be pointed out in books of architecture; its application to the common business of life is so various and immediate, as to fix it for ever in the memory; besides, the habit of abstraction, which is acquired by drawing the sections of complicated architecture or machinery, is highly advantageous to the mind. The parts which we wish to express are concealed, and are suggested partly by the elevation or profile of the figure, and partly by the connection between the end proposed in the construction of the building, machine, &c. and the means which are adapted to effect it.

The art of perspective requires a contemplation of the *mere picture* of external objects.

"A knowledge of perspective is to be acquired by an operation of the mind, directly opposite to what is necessary in delineating the sections of bodies; the mind must here be intent only upon the objects that are delineated upon the retina, exactly what we see; it must forget or suspend the knowledge it has acquired from experience, and must see with the eye of childhood no farther than the surface. Every person who is accustomed to draw in perspective, sees external nature, when he pleases, merely as a picture: this habit contributes much

to form a taſte for the fine arts; it may, however, be carried to exceſs. There are improvers who prefer the moſt dreary ruin to an elegant and convenient manſion, and who prefer a blaſted ſtump to the glorious foliage of the oak.

"Perſpective is not, however, recommended merely as a means of improving the taſte, but as it is uſeful in facilitating the knowledge of mechanics. When once children are familiarly acquainted with perſpective, and with the repreſentations of machines by elevations, ſections, &c. prints will ſupply them with an extenſive variety of information; and when they ſee real machines, their ſtructure and uſe will be eaſily comprehended. The noiſe, the ſeeming confuſion, and the ſize of ſeveral machines, make it difficult to comprehend, and combine their various parts, without much time, and repeated examination; the reduced ſize of prints lays the whole at once before the eye, and tends to facilitate not only comprehenſion, but contrivance. Whoever can delineate progreſſively as he invents, ſaves much labour, much time, and the hazard of confuſion. Various contrivances have been employed to facilitate drawing in perſpective, as may be ſeen in "Cabinet de Servier, Memoirs of the French Academy, Philoſophical Tranſactions, and lately in the Repertory of Arts." The following is ſimple, cheap, and portable.

Young perſons who underſtand perſpective may acquire much mechanical knowledge from drawings.

"Plate XVI. Fig. 1. A, B, C, repreſent three mahogany boards, two, four, and ſix inches long, and of the ſame breadth reſpectively, ſo as to double in the manner repreſented. Fig. 2, the part A is ſcrewed, or clamped to a table of a convenient height, and a ſheet of paper, one edge of which is put under the piece A, will be held faſt to the table. The index P is to be ſet (at pleaſure) with its ſharp point to any part of an object which the eye ſees through E the eye-piece.

Deſcription of the inſtrument for fixing the points in perſpective views.

"The machine is now to be doubled as in Fig. 2, taking care that the index is not diſturbed; the point, which was before perpendicular, will then approach the paper horizontally, and the place to which it points on the paper muſt be marked with a pencil. The machine muſt be again unfolded, and another point of the object is to be aſcertained in the ſame manner as before; the ſpace between theſe points may be then connected with a line; freſh points ſhould then be taken, marked with a pencil, and connected with a line; and ſo on ſucceſſively till the whole object is delineated."

Its uſe.

The

Method of using a plain graduated ruler for taking views, &c.

The above machine affords a delineation which is ſtrictly accurate: but I take this opportunity of mentioning one ſtill more portable, though leſs exact, which may be uſed in taking ſmall ſketches in the field; where the table, and fixed ſheet of paper cannot always be ſuppoſed at hand. I do not know the contriver. It is merely a ſtrait flat ruler, having a diviſion of inches and ſmall parts (or any other diviſion) on its edge. A ſtring is faſtened to the middle of the ruler by paſſing it through a hole, and tying a knot on the other ſide; and at the other end of the ſtring there is a ſmall bead or knot to be held in the mouth. The length of the ſtring may be adjuſted at pleaſure; and when the ruler is uſed, it is held up at right angles to the ſtretched ſtring, ſo that its edge, as ſeen by one eye, may apply to any two objects; between which it will ſhew the diſtance to be afterwards transferred upon the paper by a ſcale, or by eſtimate.

Farther obſervations.

In this uſe of a graduated rule, it is moſt convenient and accurate to ſelect ſome one object in the picture for the point of ſight, and to meaſure all the diſtances from thence from the middle or beginning of the diviſions where the direction of the ſight is at right angles to the rule. And as this ſimple inſtrument does not give the inclinations, it may be beſt always to meaſure parallel or perpendicular to the horizon, and eſtimate the reſt. Indeed, the contrivance muſt be conſidered only as a ſubſtitute for the uſual method of eſtimating, and may be principally uſeful to aſſiſt in acquiring a correct judgment in this reſpect.

W. N.

IX.

Concerning the new Planet CERES. *In a Letter from the Rev.* WILLIAM PEARSON, *including an Extract of a Letter from the* BARON DE ZACH *to Mr.* EDWARD TROUGHTON.

To Mr. NICHOLSON.

SIR,

Parſon's Green, March 9, 1802.

Introductory remarks. Popular account of the manner of applying obſervations.

AS the diſcovery of the new planet Ceres is an event which has already engaged the attention of the principal aſtronomers of Europe, and will continue to be a ſubject of diſcuſſion until

until the elements of its orbit ſhall be accurately aſſigned, it may be acceptable to many readers of your Journal to have a popular detailed account laid before them of the manner of applying obſervations to determine the whole period and form of the orbit, in order that they may comprehend, and conſequently feel an intereſt in, the peruſal of the different notices which will, moſt probably, be publiſhed from time to time in different countries.

When a heavenly body is firſt ſuſpected to be a planet, the ſuſpicion ariſes either from its apparent aſpect compared to a ſtar when viewed in a teleſcope, as was the caſe with Geo. Sidus, or otherwiſe on account of an obſerved change of relative ſituation, as was the caſe with the new planet Ceres when compared with ſome ſmall ſtars in its neighbourhood: the firſt thing to be done after the ſuppoſed diſcovery of a new planet is, to aſcertain its right aſcenſion and declination, and from thence its geocentric longitude and latitude; the means uſed for doing which need not be deſcribed here.

How a planet is diſcovered—by its aſpect through the teleſcope;—or its motion.

A ſeries of obſervations, and correſponding calculations, are uſually continued at ſucceſſive intervals, until the newly diſcovered body has advanced or receded through ſuch a portion of the ecliptic as to afford data for eſtimating its daily velocity when compared to that of the earth: now, if a ſeries of obſervations could be made by an obſerver in the ſun, the arc paſſed through by the planet would bear the ſame proportion to the interval of time elapſed between the firſt and laſt obſervations, that a circle does to the whole period, provided the motions were *equable;* and, if the motions were unequable, the obſerved progreſs at equal intervals would ſhew whether the inequalities were increments or decrements, and conſequently whether the planet was approaching the perihelion or aphelion point. But the obſerved places are *geocentric*, and muſt therefore be converted into heliocentric, to gain thoſe obſervations which an obſerver placed in the ſun would make; for which purpoſe the proportion to be uſed will be, by a ſimple caſe in plane trigonometry, *as the diſtance of the planet* (to be at firſt aſſumed) *from the ſun* : *is to the ſine of its obſerved elongation*, or angular diſtance from the ſun (or its ſupplement) : : *ſo is the earth's diſtance* from the ſun, taken from the tables, : *to the* ſine of an angle which is the *difference* between the heliocentric and geocentric places, and which is called the *parallax of the orb.*

Subſequent obſervations. Theſe obſervations being made *from the earth* or geocentric, muſt be converted into heliocentric reſults, or ſuch as would appear *from the ſun.*

Manner of doing this.

The

From an helio-centric arc paſſed over in a known time, its *approximate period* is deduced;—and thence by Kepler's law a *correcter diſtance*. And the computations may thus be repeatedly amended.

The heliocentric arc, which the planet has paſſed through in a given time, being thus *nearly* aſcertained, the whole *approximate period* is next found on a ſuppoſition of equal arcs being paſſed through in equal times; which will afford data for determining a ſomewhat more accurate diſtance of the planet from the ſun, by the well known law of Kepler, by which the ſquares of the periodic times are analogous to the cubes of the mean diſtances of the earth and any other planet reſpectively: an *approximate* diſtance being thus obtained may, in the next place, be ſubſtituted for the *aſſumed* diſtance, and the parallax of the orb be determined a ſecond, and even a third time in this way; by means of which repetitions an approximate period, and a correſponding approximate diſtance, will be obtained. Theſe will vary more conſiderably from the truth the greater the diſtance of the obſerved arc is from one of thoſe two points of the orbit where the planet has a mean motion; which points are always nearer to the aphelion than to the perihelion point, by a quantity which depends upon the eccentricity. If therefore it ſhould ſo happen that a new planet, at the time of its firſt diſcovery, were at, or very near, its mean diſtance, the whole period and diſtance obtained from a few of the firſt obſervations would be pretty accurate.

Errors from eccentricity,

corrected by determining the elementary points of the orbit, &c.

The next ſtep to be taken is to determine the increments or decrements of motion, when a number of geocentric are changed into heliocentric places, and thus to trace the points in the ecliptic where the velocity is a *maximum*, where it is a *minimum*, and where it is a *mean*. Theſe will ſhew where the perihelion and aphelion points are, and alſo the place of equated anomaly at an inſtant when the equation is a maximum; theſe data afford the means of calculating the greateſt equation and correſponding eccentricity; but before they can be aſſigned with accuracy, a conſiderable time muſt elapſe to afford the aſtronomer an opportunity of obſerving a few ſucceſſive oppoſitions or conjunctions and ſtationary points, for the purpoſe of correcting the approximate elements, and of determining the true ſhape and poſition of the orbit.

Determination of the obliquity and place of the nodes.

In the mean time the geocentric latitudes, gained by obſervation, muſt be alſo converted into heliocentric latitudes, in order to determine the nodes or points where the two oppoſite ſides of the orbit croſs the ecliptic. For doing this the analogy

is

is—*As the sine of the planet's elongation from the sun*: — *is to the angle of commutation* (or difference between the helio. longitudes of the planet and of the earth)—*So is the tangent of the geocentric lat*: *is to the tangent of the heliocentric latitude.*

From this account of the means necessary to be used in ascertaining the precise nature of the orbit of a newly discovered planet, it will be naturally inferred that the elements already assigned to the orbit of the new planet Ceres cannot be very accurate, allowing them to be duly proportioned to one another, because an error in any one of its elements renders all the rest erroneous; and there has not yet elapsed, probably, much more than one-fourth of an entire period since its discovery, nor has it yet been in the most essential portions of its orbit for affording the best data. Hence a continuation of accurate reports concerning this planet ought to be publicly recorded from time to time, in order that a comparison of different and distant observations may afford the requisite data for ultimately settling its elements with accuracy. I shall therefore make no apology for laying before your readers an extract from a letter of Baron Von Zach to his friend and correspondent, Mr. Edward Troughton, mathematical instrument-maker, of Fleet Street, who has very obligingly put it into my hands, with permission to make what use of it I may think proper. The letter is unusually long, and full of interesting matter; but I shall confine myself, in this communication at least, to those parts of it which principally relate to the new planet, and the subjects connected with it.

From the preceding account it follows that no elements of the new planet can yet be accurate.

Extract of a Letter from the Baron Von Zach to Mr. Edward Troughton.

Letter from Von Zach.

Gotha, January 28, 1802.

" ********** YOU have heard perhaps, dear friend, that I was so lucky as to discover again Mr. Piazzi's planet, called now, in honour of the king of Naples, *Ceres Ferdinandea*. I found this little planet first on the 7th of December last year, just between the head and the north wing of Virgo, in 178° 33¼′ right ascension, and 11° 41½′ declination N. An astronomical friend of mine, Dr. Olbers, in Bremen, found

Re-discovery of the planet Ceres.

found the planet on the 2nd of January between the N.° 20 ♍ and ρ Virginis. I have not heard that this little planet, which resembles a ftar of the ninth magnitude in fize, has been feen in France and England. I have already given information of my difcovery to the prefident of the Royal Society, Sir Jofeph Banks. I have the honour to fend you here my obfervations of this planet.

Table of obfervations.

SEEBERG OBSERVATORY.

	Mean Time.			Ap. Right Afcen.			Declination.	
1801. 7 Dec.	18h	48′	10,3″	178°	33′	30,6″	11°	41½′ N.
31	17	38½	: :	184	44	: :	11	5
1802. 11 Jan.	17	3	17,4	186	45	49,95	11	15
16	16	46	25,6	187	27	53,25	11	26
22	16	25	23,9	188	6	25,8	11	44
25	16	14	32,9	188	20	39,15	11	54
26	16	10	53,7	188	24	49,5	11	57

" If you, or your aftronomical friends, wifh to look at the new planet Ceres, which is rather difficult, this heavenly body being fo fmall, I fend you here a little ephemeris, which will direct you in the refearch of this planet. With my eight feet tranfit inftrument, by the late Mr. Ramfden, and with 200 magnifying power, I could not perceive the leaft mark of a difk. Perhaps Mr. Herfchel will fee more: perhaps he will difcover fome fatellites to Ceres.

Ephemeris.

Pofition of the Ceres Ferdinandea for Midnight, in the Seeberg Obfervatory.

	Right Afc.			Declin.		Rt. Af. in Time		
1802. Feb. 2	6s	8°	43′	12°	31′	12	34	53
5	6	8	45	12	47	12	35	1
8	6	8	44	13	4	12	34	55
11	6	8	38	13	22	12	34	33
14	6	8	29	13	41	12	33	57
17	6	8	16	14	1	12	33	5
20	6	8	0	14	21	12	31	59
23	6	7	40	14	42	12	30	40
26	6	7	17	15	3	12	29	8
March 1	6	6	50	15	24	12	27	22

Difficulties in the obfervations.

" How much I want your four feet circle in my obfervatory, my prefent perplexity will fhow you. Hitherto I have obferved all my zenith diftances with a four feet Dollond's quadrant,

quadrant, and a circle of two feet from Cary; the telefcopes have little aperture, fo that I cannot fee the new planet Ceres, and confequently I cannot obferve the zenith diftances: as the air all January was very hazy and cold, I had much ado to fee the planet in my very excellent tranfit inftrument. I muft therefore wait till the planet fhall increafe in brightnefs, which yet will not go farther than to a ftar of the feventh magnitude. In the mean time I hope that by very clear weather I may diftinguifh the planet in my old quadrant, or in my little circle, and that this faint body will fupport the illuminating of wires, which has not been the cafe hitherto. This is the reafon that I could not obtain one exact declination, but my right afcenfions of Ceres are extremely exact. This difficulty will certainly never take place in my obfervatory when I once get your four feet circle. * * * * * * *

"As foon as the inftruments arrive, I fhall give you notice of it: in the mean while I have the honour to fubfcribe myfelf, with the greateft regard and higheft efteem, which the reigning Duke of Saxe Gotha very fincerely fhares with me,

Moft honoured Friend,

Your moft obedient Servant,

And devoted Friend,

FRANCIS BARON DE ZACH,

Lieutenant-Colonel, and Director of Seeberg Obfervatory.

"Before I finifhed this letter, I had two obfervations more of the new planet Ceres, viz.

Lateft obfervations.

		Rt. Afc. Ceres.	Decl. Ceres.
28 Jan.	16h 3' 29" M. T.	188° 31' 37,85"	12° 9' 41,3"
		tolerably well at the quadrant.	
29	15 59 43,7	188 34 18,13	12° 14'
			only gueffed."

This extract affords me an opportunity of illuftrating the account, which has been juft given, of the application of two diftant obfervations of a planet for determining the heliocentric arc that has been run through in a given time: if I take the right afcenfion of Ceres for the 1ft of Jan. 1801, as given by

Application of the inftructions before detailed, to the obfervations on Ceres.

 Piazzi's

Piazzi's first observation, and also for the 26th of Jan. 1802, as given by the last observation of Von Zach in the present extract, and project the orbits of the earth and Ceres according to the eccentricity, mean distance and place of the aphelion of Gauss, as given in the extract from the Moniteur in the different journals, we shall have the figure represented in Plate XIII. Fig. 1, which is thus constructed, viz.

Construction of the orbits of the earth and Ceres, with the geocentric and heliocentric places of the latter, &c.

Supposing the point S to be the sun, describe, with any radius, the circle marked ♈, ♉, ♊, &c. for the ecliptic, and conceive it to be at an *infinite distance;* draw an occult line from S to ♋ $9\frac{1}{2}°$, the earth's aphelion, and another to ♒ $26\frac{1}{2}°$, the aphelion of Ceres; take 10 from a scale of equal parts, and describe with that radius the innermost circle A ⊕ P from a point $\frac{1}{60}$ of the radius from S towards A in the occult line, and another circle with a radius of 27,6 of those equal parts from the point C $\frac{1}{10}$ of the said radius from S in the second occult line, and these circles, which are eccentric with respect to the sun at S, will very nearly represent the required orbits of the earth and Ceres, in both which A represents the aphelion, and P the perihelion points. In the next place find the two points in the earth's orbit, which are diametrically opposite the sun's place for Jan. 1, 1801, at 9^h P. M. and Jan. 27, 1802, at 4^h A. M. which will be ♋ 11° 1′ 33″ and ♌ 6° 35′ respectively, and mark them as in the figure with their dates; after this mark the geocentric right ascensions of Ceres (denoted by the letter C with a cross beneath) for Jan. 1, 1801, and G at ♉ 21° 47′ 48″, according to Piazzi, and for Jan. 26, 1802, at G at ♒ 8° 21′, according to Von Zach, and draw the two dotted lines from S to each; then if dotted lines be drawn parallel to these two lines from the earth (⊕) in Jan. 1801 and 1802 respectively, until they touch the orbit of the planet Ceres, these last lines will mark the geocentric *apparent places* of this planet in its *orbit*, of which the dotted lines from S to the ecliptic denote the *measure;* and, lastly, if lines be drawn from S through the geocentric places to the ecliptic to H and H, they will indicate the heliocentric longitudes, and the arcs contained between the heliocentric and geocentric measures will be on each day the measure of what is called the parallax of the orb.—This method of converting geocentric into heliocentric places by projection is capable of great accuracy, and is, so far as I know, original.

The method of converting geocentric portion into heliocentric

The

The reaſon of drawing the occult *parallel* lines, which may not occur to the reader, is this; as the ecliptic is conſidered to be an infinite diſtance, ſo that the whole orbit of the earth, if ſeen from it, would appear only as a point, a line drawn to it from the ſun, and another parallel thereto from the circumference of the earth's orbit, will, to an eye placed in the ecliptic, even if it were only at the diſtance of a ſtar, appear *coincident* on the *ſame line;* hence if a line be drawn from the earth to the planet in its orbit, let their reſpective ſituations be what they may, another line drawn parallel to that from the ſun, till it reaches the ecliptic, will ſhew the geocentric meaſure therein the ſame as if that meaſure were taken in a graduated ecliptic deſcribed from the earth as a center, and having all its diviſions exactly parallel to thoſe of the ecliptic deſcribed from the ſun as a center.

by projection is conſiderably accurate;—and new.

Indeed, I have tried this projection with the geocentric and heliocentric places of ſome of the other planets taken from an ephemeris, and find it extremely accurate as well as eaſy.

Tried with the other other planets.

From this projection of the heliocentric and geocentric places of Ceres, it appears evident that the diſtance from the earth to it was much greater laſt ſummer when the earth was at P, than it is at preſent, and it is equally evident, that as the velocity of the earth is greater than that of the new planet, and as they were both moving in the ſame direction at the latter period in the projection, the diſtance will continue to diminiſh until both the planets are in the ſame ſtrait line as ſeen from the ſun, agreeably to what is ſaid in the extract from Von Zach's letter; for the apparent magnitude is in an inverſe ratio of the diſtance, ſo that as the diſtance diminiſhes, the apparent diſk increaſes. The reaſon alſo appears clear, why there was very little apparent motion of the new planet in the laſt month as ſeen from the earth, for in this part of the earth's orbit Ceres would *appear* to have a retrograde motion, provided it were at *reſt*, but its forward motion in a certain degree balanced that, and produced the effect of little or no apparent motion at all:—on the 6th or 7th of Feb. laſt it was ſtationary, and has been ſince retrograde in a ſmall degree.

Remarks from the projection of the relative motions and apparent magnitude of the planet.

termination of the heliocentric arc passed through,

In order to ascertain by calculation the heliocentric arc passed through in 390,3 days, the time contained between Jan. 1, 1801, at 9 P. M. and Jan. 27, 1802, at 4 A. M. the heliocentric longitudes of the new planet may be ascertained thus;

	S	°	′	″
1801, Jan. 1ᵈ 9ʰ sun's long.	9	11	1	33
Right ascension of Ceres	1	21	47	48
Elongation	7	19	13	45

Then at 27 (supposed distance)	1,43136
Is to sine of 49° 14′	9,87931
So is 10 (earth's distance) .	1,00000
	10,87931
	1,43136
To sine of paral. of orb. 16° 17′	9,44795

	S	°	′
Again 1802, Jan. 26ᵈ 16ʰ sun's long. . . .	10	6	35
Right ascension of Ceres	6	8	24
Elongation	3	28	11

Then as 27	1,43136
Sine of 61° 49′ (supplement)	9,94519
So is 10	1,00000
	10,94519
	1,43136
To sine of paral. of orb. 19° 3′	9,51383

Geocentric place of Ceres.	S	°	′	″		°	′
Jan. 1, 1801 . .	1	21	47	48	Parallax	16	17
Jan. 26, 1802 . .	6	8	24	49	Ditto	19	3
Whole geocentric arc	4	16	37	1		35	20
Deduct sum of parallaxes	1	5	20				
Whole heliocentric arc	3	11	17	1 in 390,3 days.			

and thence of the period 1387,27 days,

Now, if the motion of the planet were equable throughout its orbit, we should have the whole tropical period from this ratio, as 101° 17′ 1″ : 390,3ᵈ : : 360° : 1387,27 days, but the portion

portion of a circle, which has been paſt ſince Jan. 1, 1801, is that in which the velocity has been above a mean velocity; therefore the period thus obtained is ſhorter than the true period by a quantity which depends upon the equation of the center, which has not yet been examined. At preſent want of leiſure, and the length of this paper, render it neceſſary that I ſhould defer entering upon the other particulars, which remain to be diſcuſſed, ſome of which, it is preſumed, will be found intereſting; for I propoſe to ſhow that ſome of the elements aſſigned by Gauſs, which are conſidered as the moſt accurate, are not in due proportion to one another. This diſcuſſion will therefore conſtitute the ſubject of another communication. In the mean time we may infer, either that the whole period is ſhorter than has been prematurely determined, or otherwiſe that the greateſt equation ſhould be much greater than the eccentricity at preſent aſſigned requires. Indeed, it will hereafter appear that the eccentricity, given from the Moniteur in the different journals, is almoſt *two-thirds* too little to correſpond to the equation which has been attributed to the new planet.

which, for rea[ſons] given, is too ſhort,

though the period generally given muſt eith[er] be too long; or the greateſt equation muſt exceed what the aſſigned excentricity requires.

I am, SIR, as uſual,

Your's very reſpectfully,

W. PEARSON.

The early obſervations of Profeſſor Piazzi, to which Mr. Pearſon refers, are given by the profeſſor in the following tables. The computations in the latter table were made from elements which, at the preſent time, would require to be greatly amended; but I have not choſen to omit them. W. N.

Early obſervations of Piazzi.

Table

Table of the Mean Time, Right Afcenfion, and Declination of the new Star as obferved; together with the Longitude of the Sun, and the Logarithm of its Diftance from the Earth.

ht af- fions and linations Ceres . and . 1801.

Month	Days of the Month.	Ten Thoufandths of the day in mean Time.	Right Afcenfion.			Declination.			Sun's Place.				Log. Diftance of ⊕ from ☉
January.	1	3635	51°	47′	48·7″	15°	37′	43·5″N	9ˢ	11°	1′	33·1″	9,992617
	2	3606		43	27·7:		41	5·5		12	2	31·7	9,992629
	3	3577		39	36::		44	31·6		13	3	30·2	9,992641
	4	3547		35	47·2		47	57·6		14	4	29·0	9,992652
	10	3378		23	1·5:	16	10	32·0		20	10	29·5	9,992768
	11	3350		22	26·0		14	30, eft.		21	11	29·5	9,992794
	13	3295		22	34·5		21	49·5		23	14	28·0	9,992848
	14	3268		22	55·8		27	5·7		24	14	27·3	9,992882
	17	——		27	35::		40	13·0		——	——	——	——
	18	——		28	45::		——	——		——	——	——	——
	19	3136		32	2·2		49	16·1		29	19	14·1	9,993060
	21	3084		38	34·0		58	35·9	10	1	21	2·5	9,993151
	22	3059		42	21·3	17	3	18·5		2	21	55·1	9,993196
	23	3033		46	43·5		8	5·5		3	22	46·4	9,993242
	24	——		51	45::		——	——		——	——	——	——
	28	2909	52	13	38·3		32	54·1		8	26	45·5	9,993522
	30	2860		27	2·1		43	11·0		10	28	10·6	9,993645
	31	2837		34	18·8		48	21·5		11	28	55·5	9,993708
February.	1	2813		41	48·0:		53	36·5		12	29	36·6	9,993773
	2	2789		49	45·9		58	57·5		13	30	17·0	9,993851
	4	——	53	7	45::		——	——		——	——	——	——
	5	2719		15	40·5	18	15	1·0		16	32	13·9	9,994083
	8	2650		44	37·5		31	23·2		19	35	2·2	9,994328
	11	2583	54	16	23·1		47	58·8 N		22	35	41·3	9,994588

N. B. The obfervations marked with two points (:) are a little doubtful; thofe marked with (::) very uncertain.

Table

Table of the Geocentric Longitudes and Latitudes of the new Star, both by Obfervation and Calculation; together with their Differences.

Longitudes an latitudes of Ceres, Jan. Feb. 1801.

Days of the Month.	Geocentric Longitude. Obfervat.	Geocentric Longitude. Calculat.	Differ.	Geocentric Latitude. Obfervat.	Geocentric Latitude. Calculat.	Differ.
1	1s 23° 22′ 58·5″	1s 23° 21′ 59·2″	—59·3″	3° 6′ 32·4″	3° 6′ 50·2″	+17·8″
2	19 44·8	18 40·2	—64·6	2 13·1	2 27·7	+16·6
3	16 49·3	15 47·1	—62·2	2 57 58·9	2 58 63·3	+ 7·4
4	14 16·5	13 18·9	—57·6	53 44·5	53 48·4	+ 3·9
10	7 59·4	7 19·5	—39 9	28 50·9	28 31·8	—19·1
11	8 25·7	7 43·4	—42·3			
13	9 58·0	9 38·9	—19·1	16 49·0	16 21·0	—28·0
14	12 1·6	11 32·9	—29·0	12 47·1	12 23·2	—23·9
19	25 49·4	25 51·5	+ 2·1	1 53 28·3	53 1·3	—27·0
21	34 21·8	34 23·4	+ 1·6	45 58·9	45 31·6	—27·3
22	39 1·8	39 6·7	+ 4·9	42 18·7	41 51·3	—27·4
23	44 15·6	44 17·4	+ 1·8	38 39·2	38 12·3	—26·9
28	24 15 16·0	24 15 28·1	+12·1	20 58·7	20 32·0	—26·7
30	30 5·4	30 23·0	+17·6	14 5·3	13 43·5	—21·8
31	38 8·6	38 20·5	+11·9	10 45·0	10 22·4	—22·6
1	46 19·6	46 38·0	+18·4	7 23·8	7 3·6	—20·2
2	54 55·6	55 12·8	+17·2	4 0·7	3 47·0	—13·7
5	25 22 43·5	25 22 52·8	+ 9·3	0 54 19·0	0 54 9·6	— 9·3
8	53 17·9	53 15·6	— 2·3	44 42·7	44 50·9	+ 8·2
11	26 26 26·1	26 26 24·8	— 1·3	35 47·9	35 50·4	+ 2·5

X.

Entertaining chemical Experiments; with Notices of various new Facts and Obfervations refpecting the Products of Nature and Art. By Mr. Frederick Accum. *Communicated by the Author.*

1. *Production of phofphorated Hidrogen Gas* *.

As the ufual method of obtaining phofphorated hidrogen gas, and exhibiting its fpontaneous inflammation, requires a confiderable fhare of attention and manual dexterity, the amateur of chemiftry will deem it not fuperfluous to have no- **Eafy method of producing phofphorated hidrogen.**

* This and the five following Experiments were firft noticed by Mr. Davy, who exhibited them in his lectures at the Royal Inftitution.

ticed

ticed here the method of producing this gas in a more eafy, expeditious, and economical manner, by merely prefenting phofphorus to nafcent hidrogen.

Zinc, fulphuric acid, and phofphorus, afford the gas; For this purpofe, let water be decompofed in the ufual manner, by means of zinc and fulphuric acid, and add to the mixture a quantity of phofphorus. The hidrogen evolved will diffolve part of the phofphorus; phofphorated hidrogen gas will be produced, and take fire at the furface of the fluid, fo long as the decompofition of the water is made with confiderable rapidity. But the gas produced in this procefs burns with a more lambent flame than that obtained in the ufual manner, probably on account of containing a larger quantity of hidrogen. The experiment is neverthelefs brilliant; for the gas is difengaged in fmall bubbles, which cover the whole furface of the fluid; they difengage themfelves rapidly, new ones are produced, and the whole fluid refembles a well of fire.

which takes fire fpontaneoufly, and continues to burn in a beautiful manner.

Particular inftructions. For the fuccefs of this experiment, it is effential that the water, during the action of its decompofition, be confiderably heated, which may be effected by a copious addition of fulphuric acid, and that the phofphorus be prefent in a confiderable quantity. Half a part of phofphorus cut into fmall pieces, one of granulated zinc, three of concentrated fulphuric acid, and five of water, anfwer this purpofe exceedingly well.

2. *Phofphorated Hidrogen Gas burns with a green Light in nafcent hyperoxiginifed Muriatic Acid Gas, under the Surface of Water.*

Phofphuret of lime, hyperoxigenifed muriate of potafh, phofphorus, and fulphuric acid, produce *fire under water.*

Put into an ale-glafs, or Florence flafk, one part of phofphuret of lime, broken into pieces of the fize of a pea [not in fmall fragments or in powder], and add to it half a part of hyperoxigenifed muriate of potafh. Fill the veffel with water, and bring carefully into contact with the materials at the bottom of the fluid, three or four parts of concentrated fulphuric acid. This may moft conveniently be done, by letting the acid fall through a long-necked funnel, reaching to the bottom of the veffel, or by caufing it to pafs down the fides of it. As foon as the decompofition of the water, and that of the hyperoxigenifed muriate, takes place, flafhes of fire dart from the furface of the fluid, and the phofphuret illuminates the bottom of the veffel with a beautiful green light.

3. *Combuftion*

3. *Combuſtion of phoſphorated Hidrogen Gas, by the Admixture of hyperoxigeniſed Muriatic Acid Gas.*

If phoſphorated hidrogen and hyperoxigeniſed muriatic acid gas be mixed together, fire and flame are produced; and ſimple muriatic acid, water, and phoſphoric acid, are formed. Flame by mixture of two gaſes.

4. *Combuſtion of inflammable Subſtances in naſcent hyperoxigeniſed Muriatic Acid Gas.*

Put into a wine-glaſs one part of hyperoxigeniſed muriate of potaſh perfectly dry; pour on it, two of colourleſs ſulphuric acid of commerce: a violent action will take place, and hyperoxigeniſed muriatic gas becomes evolved. If, during the extrication of this gas, one part of ſulphuric ether, alcohol, or oil of turpentine, be ſuffered to fall into the gas, an accenſion takes place, accompanied with a crackling noiſe. Numerous combuſtions in hyperoxigeniſed muriatic gas:

In this manner not only all the inflammable fluid bodies, but likewiſe moſt of the ſolids, ſuch as camphor, reſin, tallow, pitch, elaſtic gum, &c. may eaſily be inflamed.

5. *Combuſtion of expreſſed Oils at the Surface of Water, by Means of hyperoxigeniſed Muriatic Acid Gas.*

Put into an ale-glaſs one part of hyperoxigeniſed muriate of potaſh; add to it three or four of water, and half a part of oil of olives, or of linſeed. On adding to it four or five parts of concentrated ſulphuric acid, a violent action takes place, much charcoal becomes depoſited, and a multitude of ignited ſparks paſs through the black fluid, exhibiting a beautiful phenomenon. On adding an additional quantity of hyperoxigeniſed muriate of potaſh and ſulphuric acid, the whole maſs takes fire, and burns with a denſe yellow flame. of oils on water;

6. *Combuſtion of Phoſphorus in hyperoxigeniſed Muriatic Acid Gas, under the Surface of Water.*

Let fall into a wine-glaſs, or rather into a long cylinder, filled two thirds with water, one part of phoſphorus, and two of hyperoxigeniſed muriate of potaſh. On adding to this mixture three or four parts of ſulphuric or nitric acid, the phoſphorus takes fire, and burns vividly under the ſurface of the fluid: on agitating the mixture, ſtreams of ignited ſparks paſs through the fluid rapidly. phoſphorus under water.

Remark.

Remark.—This and the two preceding experiments require caution. The operator ought to be diftant during the affufion of the acids, or the addition of the combuftible body, which are fometimes thrown out of the veffel to a confiderable diftance.

7. *Decompofition of fulphureous Acid Gas, by Muriate of Tin.*

Decompofition of fulphuric gas by mur. of tin.

If fulphureous acid gas, and frefh prepared muriate of tin, be brought into contact, the volume of the gas becomes fpeedily diminifhed, fulphur is depofited, and the fimple muriate becomes converted into an oxigenated muriate of tin.

8. *Syrup of Violets, which has loft its Colour, regains it, when agitated in Contact with Oxigen Gas.*

Syrup of violets reftored by oxigen.

It is a fact well known to chemifts, that fyrup of violets is apt to lofe its colour by age, though not the leaft perceptible change in the faccharine folution has taken place, when expofed to light, or even when frequently agitated in contact with atmofpheric air. In order to make the difcoloured fyrup regain its primitive blue colour, nothing more is neceffary than to agitate it for a few minutes in contact with oxigen gas.

9. *Phofphorated Hidrogen Gas is decompofed by Light.*

Light decompofes phofphorated hidrogen.

Though phofphorated hidrogen gas may be kept over mercury in the dark for any length of time unaltered, this is not the cafe if the gas be expofed to light. In this fituation the union of the phofphorus and hidrogen is broken, the phofphorus becomes feparated, and cryftallizes in the veffel, and the hidrogen is left behind.

10. *Acetite of Barites is capable of cryftallizing.*

Cryftals of acetite of barites.

A folution of acetite of barites, prepared by diffolving pure barites in pure acetous acid, which had been expofed to light in an open glafs veffel, was found to be converted into a folid regular cryftalline mafs. The form of the cryftals could not be determined, on account of their extreme minutenefs.

11. *Spontaneous Reduction of Howard's fulminating Mercury.*

Spontaneous reduction of fulminating mercury.

Four ounces of Howard's fulminating mercury were placed ftill wet on a chalk ftone, expofed to the rays of the fun in a window. It was left in this fituation unobferved for at leaft three

three months. The product during this time was converted into a black brilliant powder. On attempting to collect it together into one heap, and feparating it from the paper which had been interpofed, a globule of running mercury was feen. On introducing the powder into a bottle, and fhaking it together, heat was evolved, and the whole mafs became reduced to the metallic ftate.

12. *Production of red Sulphuret of Mercury in the humid Way.*

When equal quantities of a concentrated folution of oxigenated muriate of mercury, and concentrated frefh prepared fuming hidrofulphuret of ammonia, are mingled together, a brownifh muddy precipitate is produced, which, when left undifturbed, turns yellow in three or four days, then orange, and at laft acquires a beautiful cinnabar-red colour. The fulphuret of mercury, when feparated, poffeffes all the properties of the beft vermilion which is met with in commerce. **Cinnabar in the humid way.**

13. *Air Bladder of the Carp contains common Air.*

The air bladder of the carp does not contain nitrogen, as Fourcroy, &c. affert. If the air of the bladder of this living fifh be examined, it will be found to contain atmofpheric air, and not nitrogen. **Error of Fourcroy concerning azote.**

14. *Fifh Bones contain much phofphoric Acid.*

The bones of fifh contain upwards of one fixth part more of phofphoric acid, than thofe of quadrupeds. They may therefore advantageoufly be employed for making phofphorus. **Fifh bones abound with phofphorus.**

15. *Wax from Spiders.*

The yellow matter depofited in veffels containing fpiders preferved in alcohol, is a true wax, which may be obtained from thefe animals by gently heating them. **Spiders' wax.**

16. *Fluoric Acid diffolves Lead.—Method of obtaining it pure.*

Fluoric acid obtained in the ufual manner, by decompofing fluate of lime by fulphuric acid in a leaden retort, always contains lead in folution; as may be proved by mingling the acid with a folution of water impregnated with fulphurated hidrogen gas. Tin veffels are lefs acted upon than thofe made of lead. The acid has no effect upon filver. **Common fluoric acid contains lead.**

In

How to obtain it pure.

In order to obtain pure fluoric acid, one part of fluate of lime, reduced to a fine powder, may be mixed with two of sulphuric acid, and one of water. The mixture is then to be introduced into a glafs retort, to which a glafs receiver has been previoufly luted, containing two ounces of water. Heat being then applied, the diftillation is to be carried on flowly. After the decompofition of the fluate of lime is effected, which may be known by the difappearance of the whitifh vapours in the retort, the contents of the receiver muft be filtered, and diftilled water added to the filtered fluid, till, on a new admixture of water, no further cloudinefs appears. The acid thus obtained contains no filex; it is abfolutely pure; and may be kept in glafs bottles covered within with wax, or, in preference, with a hard varnifh.

17. *Benzoic Acid exifts in Vanelloe.*

Cryftalline matter on vanelloe.

A quantity of vanelloe pods [*Epidendrum Vanilla*, L.] which had been kept wrapt up into a bladder, and furrounded with thin fheet lead for upwards of four years, were found to be covered with a white powder of a pungent faline tafte. On examining the internal parts of the wrinkled fhell of the pods, the black feeds, when viewed under the magnifier, were found to be covered over with a multitude of oblong cryftals, croffing each other in all directions.

Experiments proving it to be benzoic acid.

In order to examine thefe cryftals, one ounce of the pods were broken into fmall pieces, and boiled in a Florence flafk, with a quarter of an ounce of lime and fix ounces of diftilled water, for about ten minutes. The whole mafs was then fuffered to repofe, and the clear fluid, which was of an amber colour, and aromatic bitter tafte, was fet afide. The refidue was treated in a fimilar manner, and the fluid was added to the firft. The clear folutions were then flowly evaporated to one fourth. During this procefs a quantity of a brown adhefive fubftance feparated, which had all the properties of a true refin; its weight amounted to nine grains. On letting fall into the concentrated folution, from which this refin had been feparated, fome pure muriatic acid, the whole became turbid, and a pulverulent yellow precipitate was depofited, which increafed in quantity on heating the mixture. All the precipitate obtained in this manner was then diffolved in diftilled water, filtered, evaporated, and cryftallized; it yielded beautiful

tiful lemon-yellow cryftals, which could not be obtained colourlefs, by repeated folutions and re-cryftallizations. But after boiling them with charcoal powder, the fluid paffed colourlefs through the filter, and yielded pure benzoic acid in the form of filky needles, weighing 23 grains.

18. *Camphor does not move upon Water at low Temperatures—Phofphorus moves upon Mercury.*

If a bafon of water, upon which fmall fragments of camphor are in rapid motion, be fuddenly transferred into a freezing mixture, the rotatory motion of the atoms of camphor inftantly ceafes. Camphor motionlefs on water in a very cold atmofphere.

If a cylinder of dry phofphorus be fcraped with a knife over a faucer containing mercury free from duft, &c. the fmall particles of phofphorus which are detached, and fall upon the quickfilver, fpin in a fimilar manner like thofe of camphor placed upon clean water. Phofphorus moves on mercury;

Camphor, benzoic acid, mufk, caftor, civet, affafœtida, vanilloe, and various other odoriferous fubftances, remain motionlefs when placed upon this fluid. but camphor, &c. do not.

19. *Frogs change their Colour, and decreafe in Size, when fecluded from Light, and die when fuddenly expofed to vivid Light.*

A number of male frogs of the largeft fize, and of a beautiful yellowifh brown colour, which had been kept for upwards of fix months in a refervoir, fituated in a dark place, loft their colour during that time, became perfectly black, and had decreafed in fize to lefs than one half. Frogs become black and fmaller by keeping in the dark. A number of thefe animals, which were very brifk, and apparently in a good ftate of health, were expofed to the rays of the fun, in order to fee if they would regain their colour. Light kills them They all died, during the experiment, in about three hours. The galvanic influence could not ftimulate their dead mufcles into action. and deftroys all irritability.

A frog, which had efcaped, was found in a veffel containing a concentrated folution of potafh, freed from carbonic acid, in which, from the time of difcovery, he could not have remained longer than three hours. A frog converted by potafh into a foluble clear mafs. The animal was much diftorted, and converted into a flefh-coloured gelatinous mafs. It was beautifully tranfparent, and twice its natural fize. On pouring boiling water upon it, the whole was diffolved into a jelly.

Frogs

Habitudes of these animals in different fluids and gases.

Frogs kept in diftilled water foon become languid, and di[...] They cannot live in water impregnated with nitrous oxid[e,] nor in water holding carbonic acid gas in folution in confid[er]able quantity. They live in an atmofphere of pure nitrog[en] for five or fix days, if water be prefent. They inftantly di[e] in all hidrocarbonates; but may be kept alive in nitrous ga[s] for fome days.

XI.

The Method of cryftallizing Lime. By TROMMSDORFF *.

History of the cryftallization of lime.

THE cryftallization of lime was firft difcovered accidentally by Schaub. Bucholz alfo obtained very fine cryftals of thi[s] earth by boiling it with its muriate. I have verified this dif[-]covery; and find that lime may be obtained in cryftals equally well in winter as in fummer; with the exception, that th[e] falt prepared during the winter feafon forms cryftals which are thicker and larger.

How effected.

In order to obtain thefe cryftals, any quantity, at pleafure, of the muriate of lime is to be boiled with one fourth, or even lefs, of cauftic lime, and the fluid concentrated until in winter a drop of the folution, let fall upon a cold ftone, fhall acquire the confiftence of fyrup without cryftallizing or congealing. The fluid is then to be ftrained through a clofe cloth filter into a capfule of porcelain or earth, which is then to be covered with a fimilar capfule, or a wooden cover, in order that the cooling may be as flow as poffible. By this means very long but flender cryftals of cauftic lime are obtained, which muft be wafhed in alcohol to clear off the adherent muriate. This operation muft not be attempted with a lefs quantity than feven pounds of muriate of lime.

Refidue, after diftilling ammonia from much lime, is the cryftallized earth.

It is well known, that when muriate of ammonia is diftilled with excefs of lime, for its decompofition, part of the refidue adheres fo ftrongly to the retort, that it is almoft impoffible t[o] detach it by foftening it. This hard mafs is for the moft part formed of lime confufedly cryftallized, which is more dif[-]ficultly diffufed in water than the fame earth in powder, or [a] ftate of minute divifion.

* Journal der Pharmacie, vol. ix. part 1. p. 108.

XII.—O[...]

XII.

On the Cryſtallization of the Hidroſulphuret of Soda. By CIT. VAUQUELIN *.

BERTHOLLET, in a memoir which he communicated to the Inſtitute about four years ago, ſhewed that ſulphurated hidrogen has ſeveral properties in common with the acids, ſuch as that of reddening the tincture of turnſole, uniting with the earths, the alkalis, and the metallic oxides, and forming cryſtallizable combinations with ſome of theſe ſubſtances.

Berthollet's obſervations, that ſulphurated hidrogen performs the functions of an acid.

I have had occaſion to obſerve, a few days ago, one of the combinations of this kind, namely, that of ſulphurated hidrogen with ſoda. Having lixiviated a conſiderable quantity of ſoda, manufactured by Citizens Payen and Bourlier, to extract the carbonate of ſoda, I left the concentrated mother water in a corner of the laboratory. At the end of ſome decads, I found at the bottom of this liquor a white tranſparent ſalt, cryſtallized in rectangular tetahedral priſms, terminated by four-ſided pyramids, ſome of which were octahedral. As this is not the form aſſumed by carbonate of ſoda, I made ſome experiments to determine its nature. Its taſte was at firſt acrid and cauſtic, nearly reſembling that of the alkalis, which led me to ſuſpect that it might be cauſtic ſoda; but I was ſoon undeceived, by an inſupportably bitter taſte which ſucceeded, and by a ſlight odour of ſulphurated hidrogen gas.

Sulphurated hidrogen forms with ſoda a cryſtallizable ſalt; of an acrid, cauſtic taſte; ſucceeded by extreme bitterneſs;

It is abundantly ſoluble in water; and notwithſtanding the cauſticity, which ſeems to announce that the alkali is united with a ſlight acid, nevertheleſs it abſorbed caloric in its ſolution. Its ſolution had no colour, but the ſmell of ſulphur was ſtronger than that of the ſalt itſelf; the acids produced a lively effervefcence, and developed a very ſtrong ſmell of ſulphurated hidrogen gas. But the fluid did not become turbid: the nitric and oxiginated muriatic acids, on the contrary, formed an abundant precipitate, which, when waſhed and dried, exhibited all the characters of ſlightly hidrogenated ſulphur. The ſalt, or its ſolution, ſpread on blotting paper, ſoon aſſumed a dark green colour. Laſtly, this ſalt precipitates all the metallic oxides from their ſolutions, perfectly ſi-

very ſoluble in water, and producing heat; effervefcing with acids; depoſiting ſulphur by the nitric and ox. mur. acids; dark green as it dries; precipitates metals like an hydroſulphuret;

* Annales de Chimie, No. 122, vol. xli. p. 190.

milar to those which the same substances assume when precipitated by the artificial hidrosulphuret of soda. It does not precipitate the earths when dissolved in the acids, excepting alumine, zircone, and yttria; which proves that the alkali is perfectly saturated with sulphurated hidrogen gas.

but does not throw down the earths; whence the alkali is saturated.

This salt, when decomposed by sulphate of iron, afforded hidrosulphuret of iron, and sulphate of soda, which were obtained crystallized by evaporation and cooling of the liquor; whence this salt is a true hidrosulphuret of soda.

With sulphate of iron it affords hidrosulphuret of iron, and sulphate of soda.

The presence of this salt in the soda of Messrs. Payen and Bourlier, proves that these manufacturers did not use a sufficiently large quantity of chalk to saturate the whole of the sulphur arising from the decomposition of the sulphate of soda by charcoal; unless we suppose that it is formed during the decoction the alkali is made to undergo when extracted from the carbonate of soda.

The manufacturers did not use chalk enough, &c.

Remark.

The whole novelty of this observation consists in the property which this salt has of crystallizing, and the determination of its figure. Its other properties have been described by Citizen Berthollet with great exactness.

XIII.

Memoir respecting a new Combination discovered in Zaffre, which CIT. BRUGNATELLI *supposed to be a peculiar Acid, which he denominated the Cobaltic Acid. By* CIT. DARRACQ *.

Supposed discovery of a new acid in cobalt, by Brugnatelli.

CITIZEN Brugnatelli, in a memoir printed in the Annales de Chimie for the month of Pluviose in the year 8, page 113 †, relates various experiments upon zaffre, or the grey oxide of cobalt, from which he concluded that this substance contains a new acid. I shall describe some of these experiments, as well as certain properties attributed to his cobaltic acid.

He digested zaffre in ammonia; filtered and evaporated to dryness. The residue gave oxide and the supposed new acid, united with ammonia.

He took zaffre, which he left in digestion with ammonia. After some days maceration in the sun he obtained a fluid of a red colour, known by the name of ammoniuret of cobalt, which he filtered and evaporated to dryness. The concrete residue thus obtained appeared to him to consist of two very distinct substances, one of a deep red colour, and the other of

* Annales de Chimie, No. 121. Vol. XLI. p. 66.

† Philos. Journal, quarto, IV. 36.

a pale

a pale yellowifh tinge. The red part was foluble in water, and the yellowifh remained upon the filter. This refidue he confidered as the pure oxide of cobalt. The foluble part was evaporated, and by cooling it depofited certain fmall cryftals, which Citizen Brugnatelli inferred to be a combination of the new cobaltic acid with ammonia. The fupernatant liquor alfo poffeffed evident characters of acidity.

The author obferved, that his acid might be obtained either coloured or colourlefs, according to the methods ufed in procuring it. Evaporations made by fire left a refidue foluble in water, and afforded a cobaltic acid nearly colourlefs, whereas thofe made in the light of the fun always afforded it more or lefs red. Citizen Brugnatelli being defirous of afcertaining whether his acid was formed during the operation here defcribed, or whether it exifted ready formed in zaffre, boiled for twenty-four hours fix pounds of this fubftance in eight pounds of water. He filtered the liquid while hot and evaporated it. When it was reduced to half it became turbid; the evaporation was continued until no more than one third of the fluid remained. It was then taken from the fire and depofited a white fubftance, which was collected upon the filter. The fluid which paffed was of a bright yellow colour, with a fenfibly acid tafte; it acted in all refpects the fame as the cobaltic acid obtained by the procefs before pointed out.

It is either coloured or colourlefs.

Zaffre boiled with water afforded it.

The following are among the properties which Citizen Brugnatelli confiders as characteriftic.

Properties ftated as characteriftic of this acid.

1. It precipitates the folution of filver.
2. It precipitates lime water in a white coagulum infoluble in water, and in an excefs of acid.
3. It is feparated from its aqueous folution by alcohol.
4. It precipitates the acetite and muriate of barites.

I fhall now proceed to defcribe the experiments I have made on this fubject, and prefent to the Inftitute the confequences which I deduce from the fame.

New experiments.

Experiment 1. I formed the ammoniuret of cobalt by leaving zaffre in digeftion in the fun's light in ammonia. I was careful to agitate the mixture which was included in a matrafs, and foon obtained a red colour, that after forty-eight hours became a bright red. I obferved in this fluid a cryftallization in confiderable abundance, in the form of white brilliant needles. This cryftallization was permanent till the fun had

Experiment I. The ammoniuret was formed.

had again heated the fluid, when it became diffolved in the liquid. In this ftate the ammoniuret of cobalt was filtered, and put into a retort; in proportion as its temperature increafed it affumed a violet red colour which grew deeper and deeper, and at length had the appearance of the bright colour of red wine. When the greateft part of the ammonia was evaporated the mixture acquired a greenifh colour, and by repofe a fubftance of the fame colour as the folution was precipitated. The liquid filtered while hot and evaporated to drynefs, afforded rudiments of cryftals, of which the form could not be determined. White and brilliant parts were obferved, and the reft of the matter was of a yellowifh colour. Upon this refidue I poured water, and agitated it with a fpatula of platina. The leaft coloured portion was totally diffolved, and communicated a ftraw colour to the water. This fluid was acid, and poffeffed fome of the properties announced by Cit. Brugnatelli. I fhall prefently defcribe the experiments to which I fubjected it, the refults of which properly examined, prove that it is not an acid formed by the cobalt. The two refidues obtained in this experiment, one of a greenifh, and the other of a yellowifh colour, were proved to be not pure oxide of cobalt, as Citizen Brugnatelli affirmed, but a combination of that oxide with the arfenic acid.

The ammonia was evaporated by heat.

A precipitate fell and the filtered fluid afforded cryftals by evaporation:

Part of the refidue was taken up by water.

It was acid,

but it was not cobaltic acid.

Experiment 2. Having prepared a new quantity of ammoniuret of cobalt as in the former experiment, I fubmitted to fpontaneous evaporation in a fituation where the fun accelerated the volatilization of the ammonia. In proportion as the liquor evaporated a flocculent matter of a whitifh rofe colour fell down, which fenfibly increafed until the ammonia was evaporated. The fluid emitted no fmell, but it had preferved a rofe colour of a confiderable beauty. I filtered it to feparate the precipitate, and again evaporated the liquor to drynefs. The remaining matter was diffolved in diftilled water. This folution of a light rofe colour was acid, and poffeffed the properties of the acid obtained in the former experiment,

Exp. II. The ammoniuret of cobalt was expofed to the fun to lofe its ammonia.

Flocculent precipitate.

The clear fluid filtered and evaporated to drynefs left a matter foluble in water, which was acid, and the fame as in Exp. I.

The refidue upon the filter was carefully examined, and afcertained to be the arfeniate of cobalt.

The firft refidue on the filtre was arfeniate of cobalt.

Experiment 3. I took, as the author of the memoir himfelf did, one kilogram of zaffre, which was pulverifed till it became an impalpable powder; I boiled it with three litres of diftilled water for half an hour, and filtered the liquid while hot.

Exp. III. Powdered zaffre was

hot. The filtered fluid had a light colour, and perceptible taſte. I evaporated it in a capſule of porcelain, and towards the end of the evaporation it became turbid. I continued to evaporate until the fluid was reduced to about one hectogram by cooling, and obtained needle formed cryſtals. The mixture was filtered with agitation; the cryſtals remained upon the filter, and the fluid paſſed very tranſparent of a bright yellow colour. Citizen Brugnatelli in making this experiment has not remarked the cryſtallization; he has only obſerved a white matter which was converted to a roſe colour by the contact of the air, and ſuppoſed by him to be the oxide of cobalt. The needle formed cryſtallization which I obtained afforded a conſiderable diſengagement of arſenic as ſoon as it was heated. When carefully examined it was proved to be the arſeniate of cobalt, and not the oxide as had been announced. This fact is no doubt of little conſequence to the diſcoveries of Citizen Brugnatelli; but neverthelefs it might ſerve to ſupport my opinion, if the reſults of my experiments were not more than ſufficient to prove that the cobaltic acid has no exiſtence.

boiled in water and filtered.

Cryſtals obtained by evaporation.

It gave out arſenic by heat;

and was arſeniate of cobalt.

The liquids of the 1ſt, 2d, and 3d experiments, which proved to be acid, and of the ſame nature, were ſubjected to the following trials, and comparatively with the arſenic acid.

Examination of the acid liquors.

Firſt trial. This acid liquor is precipitated by ſulphurated hidrogen, and by the alkaline hidro-ſulphurets of a yellow colour ſimilar to opiment, or the ſulphuret of arſenic. This precipitate is in fact ſulphuret of arſenic, and not ſulphur precipitated, as Citizen Brugnatelli has ſuppoſed.

Sulphurated hidrogen throws down ſulphuret of arſenic.

Second trial. It precipitates the ammoniuret of copper of a blueiſh green colour. This property belongs to the arſenic acid. The combination is known by the name of arſeniate of copper.

It forms arſeniate of copper with the ammoniuret of that metal.

3. Sulphate of copper mixed with this fluid affords a precipitate of the ſame colour as the ammoniuret of this metal. The arſenous acid alſo poſſeſſes this property not ſo eminently as the pretended cobaltic acid, but a ſimilar precipitate may be obtained by uſing the arſeniate of cobalt formed artificially. In this caſe the reſults are the ſame, and no difference can be perceived.

Its effect on ſulphate of copper is the ſame as that of the arſeniate of cobalt.

4. It precipitates the nitrate of ſilver of a white colour. The arſenic acid alſo enjoys this property.

It alſo acts like arſeniate of cobalt on nitrate of ſilver;

5. The

and of mercury; 5. The nitrate of mercury is thrown down of a ſtraw colour; and this phenomenon is alſo preſented by the arſenic acid.

and lime water; 6. It precipitates lime water in a white coagulum inſoluble in water, but not inſoluble in an exceſs of acid, as Citizen Brugnatelli affirms; for in fact, it is re-diſſolved with the ſame facility in the pretended cobaltic acid as in arſenic acid. I think the miſtake of Citizen Brugnatelli ariſes from his not having uſed a ſufficient quantity of acid.

and acetite and muriate of barites; from impurity. 7. It precipitates the acetite and the muriate of barites. I am aſſured that this precipitation aroſe from a ſmall portion of ſulphuric acid contained in the acid called cobaltic. The arſeniate of cobalt alſo renders theſe ſolutions cloudy.

Precipitates infuſion of galls. 8. It forms an abundant yellowiſh precipitate with the tincture of nut gall newly made. It is known that the arſenic acid produces the ſame phenomenon.

9. The laſt trial. This laſt experiment appears to have been conſidered by Citizen Brugnatelli as the moſt characteriſtic of his cobaltic acid. He found that alcohol ſeparated his acid from its aqueous ſolution, and alſo that the arſenic acid diſſolved in alcohol enjoys the ſame property; by theſe proceſſes he obtained his concrete acid. This property of the cobaltic acid at firſt ſurpriſed me; but preſuming that I ſhould diſcover the explanation by examining the acid thus precipitated, I ſubjected it to the following experiments.

The pretended cobaltic acid is ſeparable from water by alcohol, and even by alcoholic ſolution of arſenic acid.

This precipitated acid gave out arſenic by heat; 1. When heated upon charcoal by the blow pipe it emitted white vapours very eaſily diſtinguiſhable to be thoſe of arſenic.

rendered borax violet; 2. A ſmall portion heated with borax communicated a violet colour.

The precipitating alcohol has acquired arſenic acid. 3. This acid being precipitated by alcohol alone I examined the liquid, after having ſeparated it by the filter. Sulphurated hidrogen produced an abundant yellow precipitate, which was found to be a ſulphuret of arſenic.

The acid precipitate was nearly inſoluble in water; 4. The ſeparated acid was ſcarcely at all ſoluble in water.

but became ſoluble by addition of arſenic acid. 5. When mixed with a few drops of arſenic acid it diſſolved completely, and reſumed its original properties.

Hence the acid of Brugnatelli was arſeniate of cobalt diſſolved From theſe trials I concluded, that the acid of Citizen Brugnatelli could be nothing elſe but the arſeniate of cobalt diſſolved in an exceſs of acid.

To put this beyond doubt I diſſolved pure oxide of cobalt in the arſenic acid; I evaporated the ſolution to dryneſs, and added

added diſtilled water to the reſidue. After agitating the mixture it was filtered. The liquid thus obtained had a ſlight roſe colour, and poſſeſſed all the properties of the cobaltic acid.

in exceſs of acid.

This acid when mixed with alcohol afforded an abundant precipitate, and the precipitate when collected preſented all the phenomena exhibited by the concrete cobaltic acid.

A direct combination of oxide of cobalt and arſenic acid, had the colour, the precipitability, and all the properties of the ſuppoſed cobaltic acid.

It appears therefore certain from the reſults I have here detailed, that the ſo called cobaltic acid has no exiſtence; that the ſimple combination of arſenic acid with oxide of cobalt has led Citizen Brugnatelli into an error, becauſe this combination with exceſs of acid, is the ſubſtance obtained from zaffre by the methods deſcribed in this memoir.

Inference.

XIV.

On the Plumb Line and Spirit Level, by EZEKIEL WALKER. *Communicated by the Author.*

Lynn Regis, March 18, 1802.

WHEN it is conſidered that the accuracy of aſtronomical obſervations depends greatly upon the exact manner of adjuſting the inſtruments, any improvement in the method of making thoſe adjuſtments may not be thought unworthy of a place in a philoſophical Journal.

Importance of the adjuſtment of inſtruments.

The plumb-line and ſpirit-level are much uſed by aſtronomers, but from the great improvements made in the conſtruction of the latter, the plumb-line ſeems to loſe that preference which it had ſome years ago.

Plumb-line and ſpirit-level.

Mr. Ludlam ſays *, " I believe the ſpirit-level cannot aſ- " certain the poſition of the axis" (of an inſtrument) " nearer " than to a minute if ſo near." And when I firſt began to uſe the ſpirit-level for determining the poſition of the axis of a tranſit teleſcope, I found it nearly in the ſame imperfect ſtate as mentioned by Mr. Ludlam: I was therefore very deſirous of having ſome better method of making this adjuſtment.

Common ſpirit level ſaid not to exceed the preciſion of one minute.

After having thought much upon the ſubject, I fixed on having a plumb-line ſuſpended from the object end of the teleſcope,

Plumb-line to a tranſit inſtrument.

* Aſtronomical Obſervations, page 42.

telefcope, to pafs over a point near the eye end. And in this manner a plumb-line was applied to it by Mr. E. Troughton, with his ufual accuracy.

Defcribed by Dr. Ufher.

This way of levelling the axis of a tranfit telefcope has fince been defcribed by Dr. Ufher in the Tranfactions of the Royal Irifh Academy.

Laid afide.

The Dr. fpeaks unfavourably of it, but many of his objections are ill founded, and it is probable that he never tried it. This method of applying the plumb-line gave me much fatisfaction at that time, but fome years afterwards it was totally laid afide for a ground fpirit-level. The fpirit-level which I now ufe is ground to a radius of 430 feet, with two indexes made of ivory, each about one-third of a minute in length, graduated to every two feconds. Thefe are made to flide upon the level, that they may be brought over the ends of the bubble at pleafure; and no other adjuftment is neceffary.

Spirit level of 430 feet radius, with indexes graduated to two feconds.

Improved method of ufing it.

My method of ufing the fpirit-level is not only more convenient than any other that I have met with, but the axis of an inftrument may alfo be examined by it without altering the perpendicular fcrew, which is a matter of fome confequence; becaufe an inftrument newly adjufted is fubject to vary for fome time after, by the changes which take place in the temperature of the air; confequently the adjufting fcrews fhould never be touched, except to make fome neceffary alteration.

Obferve one end of the bubble; reverfe the level, and obferve again. The half difference of ftation is the error; to which bring the inftrument.

My method is as follows. Having hung on the level, flide an index over one end of the bubble, and after it has remained at reft about a minute read off from the index the number of feconds *a* pointed out by the end of the bubble, then take off the level, invert the ends, and hang it on again, and if the fame end of the bubble come to the fame divifion (*a*) as before, the axis is level. But if the end of the bubble reft at any other divifion (*b*) on the index, then half the difference will be the error; and by means of the perpendicular fcrew, bring the end of the bubble half way between *a* and *b*, and the axis will be level without further trouble.

If the pofition be alfo determined by the other end of the bubble, the difference is feldom half a fecond.

The pofition of the axis may be alfo determined at the fame time, by the other end of the bubble, and if the level be a good one, the refults given by the two ends will feldom differ fo much as half a fecond.

In

In confequence of an accident which happened to my fpi-rit-level, I was under the neceffity of having recourfe once more to my plumb-line.

Plumb line refumed.

But Mr. Nicholfon's paper on page 134, Vol. I. of his Journal, (quarto) made me look upon it with a very fufpicious eye. My having been affured that my wire was of the beft quality, did not remove thofe fufpicions which that paper had raifed in my mind of its being crooked. At laft a method of trying this by experiment occurred to me, which was by reverfing the fides of the wire, thus: after having made the wire to bifect the point near the eye end of the telefcope, I turned that fide of the wire which faced the fouth, to face the north, which was done very eafily, by reverfing that piece of brafs to which the wire is fixed at the top, and at the fame time turning the plumb line half round the fame way. To my great furprize I found that my wire did not form an accurate right line by feven feconds, though it was loaded with a proper weight.

Sufpicion of flexure in the wire.

How verified.

The error from flexure was feven feconds.

I tried another piece of wire from the fame bobbin, which deviated fix feconds from a right line; I tried feveral other pieces before I found one that ftood the teft of this examination.

But after the error of a plumb-line has been thus determined, it is as good as if it were perfectly ftraight.

Remedy. But qu. if the error be invariable? W. N.

Another inconvenience that attended my plumb-line was this,—though the wire hung as near the point as poffible without touching, yet the microfcope could not be adjufted to view them both diftinctly at the fame time. An accident, however, put it in my power to remove this difficulty. Levelling the axis of the tranfit, one day, without any light in the room, except that which came through the opening in the roof behind me, I obferved the fhadow of the wire upon the telefcope. By placing the microfcope a little on one fide of the wire, I faw the fhadow very black and perfectly well defined. The fhadow and the point being on the fame plane, both objects were diftinctly feen with the fame adjuftment of the microfcope.

Difficulty of obferving the wire and the ftroke at the fame time.

Removed by ufing the fhadow of the wire.

The length of this plumb-line from the notch in which the wire refts at the top to the *point* is about 41½ inches. The wire fubtends an angle of 24 feconds at the point, and the point itfelf 52 feconds, confequently four feconds of the point are feen on each fide of the fhadow when the wire bifects the point.

Eftimate error of adjuftment by this plumb line.

Though these eight seconds measure only .00159 parts inch, yet an experienced observer will estimate with rable exactness when the parts of the point seen on side of the shadow are equal. Still it is not improbable, a second or two may sometimes escape his notice on so l a scale.

he spirit-level has the advantage of so large a scale, that a nd may be divided by the eye into many equal parts, and so easy in its application, that a few minutes only are sufficient for determining the position of the axis of an astronomical instrument, to less than a single second, as appears by the following observations.

Position of the Axis shewn by the Spirit-Level.

y the end x.	By the end y.	
″	″	″
1.75 -	- 1.7	$x+y \div 2 = 1.72$
1.65 -	- 1.85	$x+y \div 2 = 1.75$
2.1 -	- 2.2	$x+y \div 2 = 2.15$
1.7 -	- 1.77	$x+y \div 2 = 1.73$
1.6 -	- 1.6	$x+y \div 2 = 1.6$

East end of the axis dips - - - - 1.79

taken ...d. These ten observations are independent of each other, they were all taken at the same time, and none were rejected to make a show of precision, instead of shewing the truth.

rels of ... ccurate. I have had other spirit-levels ground by the same ingenious artist *, which were as good as this, though one of them was filled with spirits of wine.

* Mr. Edward Troughton, Fleet-Street, London. He in general uses ether.

XV. *Description*

XV.

Description of a Machine for raising Ore from Mines. By Mr. T. ARKWRIGHT *, *of Kendal, Westmoreland.*

A, Plate XV. is an endless chain formed of thin plates of iron, through the ends of which plates iron bolts are passed, which keep the sides of the chain at a certain distance asunder, and on which the buckets to contain the ore are suspended. B C D E, the buckets suspended on the iron bolts, G H I, three cylinders, round which the chain and buckets revolve. The two cylinders G H are placed above the shaft; the cylinder I within the mine. Their rims are so much higher than the body of the cylinders, as to admit the buckets to lie within the rims. **Construction of a machine for raising ore from mines.**

As the endless chain and buckets are moved forwards by a power applied to the axis of the cylinder G, the bolts of the chain fall into notches made at regular distances in the rims of that cylinder, which preserve the chain from slipping.

As each empty bucket passes under the axis of the bottom cylinder I, it loads itself with ore instantaneously from a large box K, constantly filling by the workmen below, which box rests on two moveable pins L, at that end furthest from the wheel, and on an iron catch M at the other. The bucket thus filled ascends to the top of the cylinder G: and, in its passage betwixt the cylinders G and H, discharges its contents into a channel or receiver placed betwixt them, from whence they slide into a cart or receptacle placed underneath the inclined trough N. The empty bucket passes over the cylinder H, descends on the opposite side under the cylinder I, and loads itself again at K, as before mentioned; the buckets regularly loading and discharging themselves, whilst the cylinder G is kept in motion.

O is a racket-wheel on the cylinder, to prevent a retrograde motion in the chain.

* From "Transactions of the Society of Arts" for 1791. p. 277. The Society gave Mr. Arkwright a premium of twenty-five guineas for this invention.

Fig. 2.

. shews, upon a larger scale, the manner in which
. K above mentioned loads the buckets. P is an iron
projecting from the endless chain, which, pressing upon
catch R, underneath the box K, occasions that part of
box next the chain to sink down, and discharge into the
-ket beneath it a quantity of ore sufficient to fill it. As the
ed bucket rises, it lifts the box K to its former place, till
operation is repeated by the next tooth upon the chain.

XVI.

Curious Properties of prime Numbers, taken as the Divisors of Unity. By a Correspondent.

To Mr. NICHOLSON.

SIR,

IN the fourth volume of your Journal, page 403, (4to.) you inserted a curious property respecting the quotients resulting from the division of an unit by all prime numbers, not less than the number 7. I shall now lay before you two other properties equally curious, arising from similar divisions, and shall be much gratified, if in the hands of some of your learned readers or correspondents, this communication should, as I trust it will, prove really beneficial.

I am, SIR,

Yours, &c.

H. G.

March 22, 1802.

PRIME NUMBERS.

Law of the quotients of unity ÷ by prime numbers.

1. The first half of the quotient figures arising from the division of an unit by any prime number, not less than the number 7·, will be the complements of the other half, to the number 9·.

EXAMPLE

Example I.

$$1 \div 7 = \cdot\dot{1}4285\dot{7}$$

or

$$1 \div 7 = \left\{ \begin{array}{l} \cdot 1\ 4\ 2 \quad .\ .\ \text{1st.} \\ \ \ 8\ 5\ \dot{7} \quad .\ .\ \text{2d.} \end{array} \right\} \text{half.}$$

$$\text{Quotient figures added} = \quad 9\ 9\ 9$$

Example II.

$$1 \div 29 = \cdot\dot{0}34482758620689655172413793\dot{1}$$

or

$$1 \div 29 = \left\{ \begin{array}{l} \cdot\dot{0}3448275862068 \quad . \ \text{1st.} \\ \ \ 965517241379\dot{3}1 \quad . \ \text{2d.} \end{array} \right\} \text{half.}$$

$$\text{Quotient figures added} \quad 99999999999999$$

Remark.—From hence it appears, that if half fuch like quotients be found by divifion, the other moiety may be obtained by a fimple addition only.

2. The firft half of the dividend figures (or fucceffive remainders) arifing from the divifion of an unit by any prime number, not lefs than the number 7, will be the complements of the other half to the prime divifor.. Law of the remainders of unity ÷ by prime numbers.

Example III.

The prime divifor being 17·.

Then $1 \div 17 = \cdot\dot{0}58823529411764\dot{7}$

or

	1	10	15	14	4	6	9	5	1ft. half of div.
$1 \div 17 =$	$\dot{0}$	5	8	8	2	3	5	2	1ft. half of quots.
	16	7	2	3	13	11	3	12	2d. half of div.
	9	4	1	1	7	6	4	$\dot{7}$	
Divs. added =	17	17	17	17	17	17	17	17	
Quots. added =	9	9	9	9	9	9	9	9	

EXAMPLE IV.

prime divisor being 23·.

1 ÷ 23 = ·0434782608695652173913·

or

$$\div 23^{\cdot} = \begin{cases} 1\ 10\ 8\ 11\ 18\ 19\ 6\ 14\ 2\ 20\ 16 \text{ divs.} \\ \dot{0}\ 4\ 3\ 4\ 7\ 8\ 2\ 6\ 0\ 8\ 6 \text{ quots.} \\ 22\ 13\ 15\ 12\ 5\ 4\ 17\ 9\ 21\ 3\ 7 \text{ divs.} \\ 9\ 5\ 6\ 5\ 2\ 1\ 7\ 3\ 9\ 1\ \dot{3} \text{ quots.} \end{cases}$$

rs. added 23 23 23 23 23 23 23 23 23 23 23

9 9 9 9 9 9 9 9 9 9 9

Remark.—From hence it appears, if half the dividends be nd (and they are easily obtained by the continual addition a few of the first quotient figures) the remainder regularly ceed them as complements of the former.

ervation.—The result of the divisions of an unit by the numbers 11, 31, 37, 41, &c. appears at first sight to ate from the general law above laid down, but in fact it subservient to it, as will appear from the following

EXAMPLE.

The prime divisor being 41·.

1	10	18	16	37	dividends.
·0	2	4	3	9	quotients.
40	31	23	25	4	dividends.
·9	7	5	6	0	quotients.
2	20	36	32	33	dividends.
·0	4	8	7	8	quotients.
39	21	5	9	8	dividends.
·9	5	1	2	1	quotients.
3	30	13	7	29	dividends.
·0	7	3	1	7	quotients.
38	11	28	34	12	dividends.
·9	2	6	8	2	quotients.
6	19	26	14	17	dividends.
·1	4	6	3	4	quotients.
35	22	15	27	24	dividends.
·8	5	3	6	5	quotients.

A table of the quotients and dividends arising from all prime numbers under 1000· is nearly completed, and will be at the service of the public whenever it shall appear sufficiently beneficial.

SCIENTIFIC

SCIENTIFIC NEWS.

Extract of a Letter from Mr. W. WALKER, *Lecturer on the Eidouranion, on the new Planet Ceres.*

To Mr. NICHOLSON.

42, *Conduit-ſtreet, Hanover-ſquare,*
SIR, *London, 25th March,* 1802.

THE intereſt that I find excited, both in public and in private, by the diſcovery of a new planet, induces me to offer you the lateſt obſervations by which it can be aſcertained, that the neceſſary publication of your moſt valuable Journal will admit. **New planet Ceres.**

Since the 15th of this inſtant March, I have regularly obſerved it; and by the following memorandums it will be readily found by any of your readers, who are ſolicitous, and provided with a night teleſcope, or a common ſpy-glaſs, for it is not viſible to my unaſſiſted eye. **Obſervations on the poſition of the neighbouring ſtars, by means of which the planet may be eaſily found.**

If at 9. 10. 11. 12. 1. 2. or 3. in the night, a line is drawn from Theta Lionis, through Beta or the Lion's Tail: at the ſame diſtance, and to the left, but a little above this termination, a ſmall configuration of ſtars will be perceived of the following deſcription: Two ſtars of conſiderable brightneſs will be found to form one ſide of an equilateral triangle, and a very ſmall ſtar the other point of this figure. Between theſe larger ones and the point will be ſeen two minuter ſtars, which, with the ſtar at the point, form alſo an equilateral triangle. The Ceres is paſſing through this cluſter. On the 15th inſtant the planet was to the eaſt of the ſmalleſt ſtar at the point of the triangle. I have regularly obſerved it on the 16th, 20th, 21ſt, 22d, and this evening the 25th, when it is arrived between the two weſternmoſt or largeſt ſtars of the above configuration.

By continuing this line it may eaſily be diſcovered for a few nights to come. The planet is as bright as moſt of the ſtars in its neighbourhood. But although I have obſerved it with magnifying powers from forty to above one thouſand times, I muſt heſitate in declaring it to have a defined diameter

ter or diſc, different from the neighbouring ſtars; whilſt the Georgium Sidus in its neighbourhood leaves no doubt of a deciſive magnitude.

I am, SIR, Your's, &c.

W. WALKER.

The following is the right aſcenſion and declination of the planet (Ceres), by Mr. Zach of Gotha, for the commencement of the next month *.

	Right Aſcenſion in Time.	Right Aſcenſion in Degrees.	Declination North.
April 3	12 0′ 12″	180° 3′	18° 6′
6	11 57 54	179 29	18 9½
9	— 55 45	178 56	18 10
12	— 53 46	178 26	18 9
15	— 51 37	177 59	18 5
18	— 50 20	177 35	17 59

Extract of a Letter from M. ZACH, *Director of the Obſervatory of Gotha, to* C. MECHAIN, *of the Inſtitute, Adminiſtrator of the Obſervatory at Paris.*

Diſc of the planet *Ceres.* Suſpicion of two ſatellites. Denſe atmoſphere.

M. Schroeter, at Lilienthal, obſerves with his great teleſcope the diſc of the new planet Ceres to be about two ſeconds. He alſo ſuſpects the exiſtence of two ſatellites. The planet is enveloped in a very thick atmoſphere, for it appears to be ſurrounded with much nebuloſity. I am very curious to know what Dr. Herſchel will tell us concerning it. In the mean time, I thought it proper to write this in haſte, &c. &c.

Elements.

Elements of the new planet, corrected by M. Gauſs, from the lateſt obſervations:—

Diurnal heliocentric tropical motion . . . . 770″ 7376
Tropical revolution . . . . 1681 days 12 hours 9 ſec.

Decade Philoſ. No. 15. *An.* x.

* A ſmall map of the ſtars above deſcribed was given in Mr. Walker's letter; but time did not admit of engraving it. W. N.

Spider's

Spider's Webs to form the crofs Wires in the Eye Piece of Aftronomical and other Inftruments.

The fmalleft filver wire I have heard of was rather thinner than one thoufandth part of an inch; but every fpecimen I have feen was confiderably thicker. This has not been fuppofed of much confequence in the conftruction of plumb lines; but it is very defirable that the wires in the focus of microfcopes fhould be as fine as poffible. In a defcription of inftruments in the Cabinet of the Grand Duke of Tufcany, arranged by the Abbé Fontana, and written by Ami Angand in one of the Journals de Phyfique above twenty years ago, to which I muft refer the reader to the index of that ufeful work, not having a whole fett at hand; mention is made of fpider's webs in the focus of microfcopes for meafuring; and long fince, when upon obferving that the fingle thread of the filk worm is very flender, I mentioned its ufe to my old friend J. H. de Magellan, F. R. S. he informed me, that organized fubftances had been tried in aftronomical telefcopes, and rejected, becaufe the folar image was found to burn them.

Smalleft wires.

Spider's webs ufed in microfcopes.

Silk worms threads.

I fuppofe this muft have been taken for granted by himfelf or fomebody elfe without trial; for Mr. Edward Troughton, whofe talents are indefatigably directed to every thing that can infure the precifion and accuracy of aftronomical inftruments, has for fome time applied the webs of young fpiders to the purpofe above-mentioned. He takes hold of a fhort length of the web by means of a forked ftick, and then carefully applies it to the marks on the frame or ring intended to hold it, where it adheres by the previous application of a little varnifh. Thefe lines have a beautiful afpect for their delicacy, precifion, and evennefs.

Troughton's application of fpider's webs to aftronomical telefcopes.

That the folar focus fhould not injure them is a fact of fome remark; which I fhould be difpofed to afcribe to their tranfparency, if I had not been affured by an eminent philofopher, that fine metallic wires were not affected by Parker's great lens, which inftantaneoufly fufed and burned thicker wires and maffes. What may be the theory of this? Will the afcending current of air cool a fmall wire more fpeedily than a larger, of which the internal parts bear a greater proportion to the furface? or does the extreme light ignite and perforate the

Curious fact that the burning lens does not deftroy fmall wire.

Conjectures.

ſmall wire, ſo as to eſcape with rapidity on the oppoſite ſurface, and keep down the maximum of temperature? &c. *Opera deſiderantur, non verba.* W. N.

Thick Iron burned in Oxigen.

Combuſtion of iron in oxigen.

Mr. Accum informs me, that a thick piece of iron or ſteel, ſuch as a file, may be burned in oxigen gas, if it be made very ſharp pointed, and a ſmall piece of wood be ſtuck upon its extremity, which is to be ſet on fire previous to immerſion into the gas.

Extreme accuracy of tuning Muſical Inſtruments.

Very exact proceſs for tuning muſical inſtruments.

About a year ago —— Huddleſtone, Eſq. ſhewed me an excellent monochord, with improvements for aſcertaining the preciſe lengths, and ſecuring the equable tenſion of the ſtring, which I ſhould be glad if I had an opportunity of communicating. I was much gratified by his method of tuning uniſons to a degree of preciſion which the direct power of the ear could aſſuredly never command. 1. He tunes his ſtring (*a*) (to the tone given by a fork, or otherwiſe) ſo as to have no beat or undulation, but to be as nearly uniſon as the ſenſe can diſtinguiſh; 2. he tunes another ſtring (*b*) a very ſmall quantity ſharper (or flatter) ſo as to afford an evident beat; 3. he ſets a ſmall pendulum, or bullet and thread, to vibrate with the beat; 4. he tries his fork with the laſt ſtring (*b*) and obſerves the beat by the pendulum, unaltered; 5. if the beat be ſlower than before, it follows that the firſt ſtring (*a*) is ſtill too flat, becauſe the fork is nearer uniſon with the ſharp ſtring (*b*);—if quicker, then (*a*) is too ſharp. I heard four forks ſtruck in quick ſucceſſion, which had been tuned by this proceſs; and though there was only the fundamental, with its third, fifth, and octavo, the effect was delightful.

Strange influence of organ pipes upon each other.

The ſame gentleman mentioned a ſingular, and I believe unobſerved fact, relative to the combination of muſical undulations in the air. Two contiguous organ pipes, both excellent, ſtopped diapaſan C and C ſharp, in a valuable inſtrument he poſſeſſes, govern each other, ſo that if C be firſt ſounded and held, the C ſharp will give uniſon to C whenever its key is ſtruck: but if C ſharp alone be ſounded and held, the C pipe will give C ſharp when its key is ſtruck. I will not venture to ſpeculate upon this.

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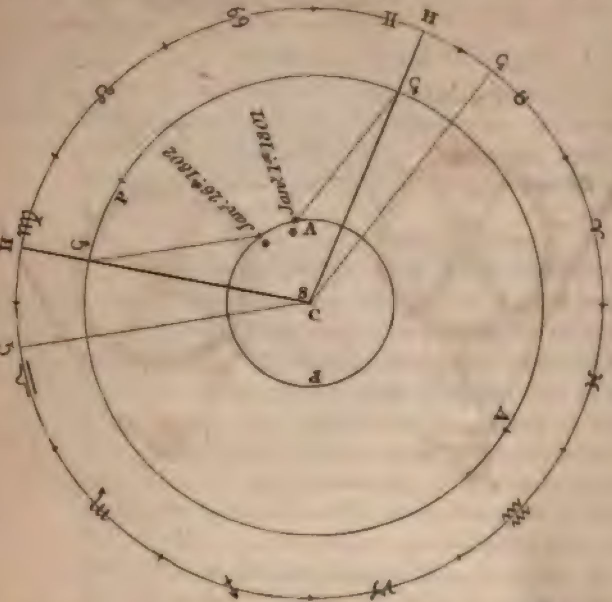

Mr. Murray on the transmission of heat thro' fluids (the vessel Ice)

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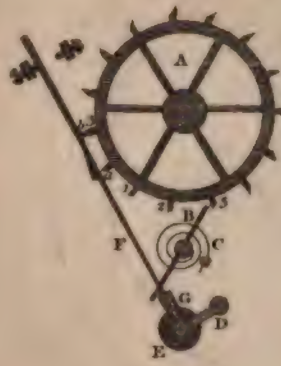
Mutlow Sc. Russell Ct.

Mr. I. Delafons's Watch Escapement.

Fig. 5.



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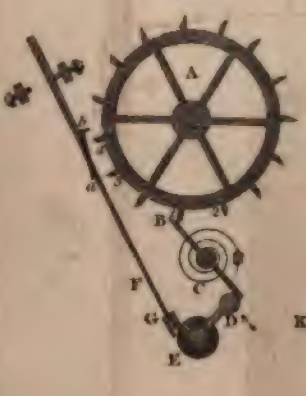

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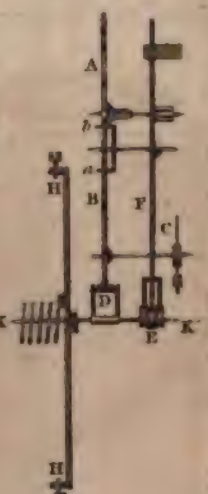

Fig. 4.

Mr. T. Arkwright's Machine for raising Ore from Mines.

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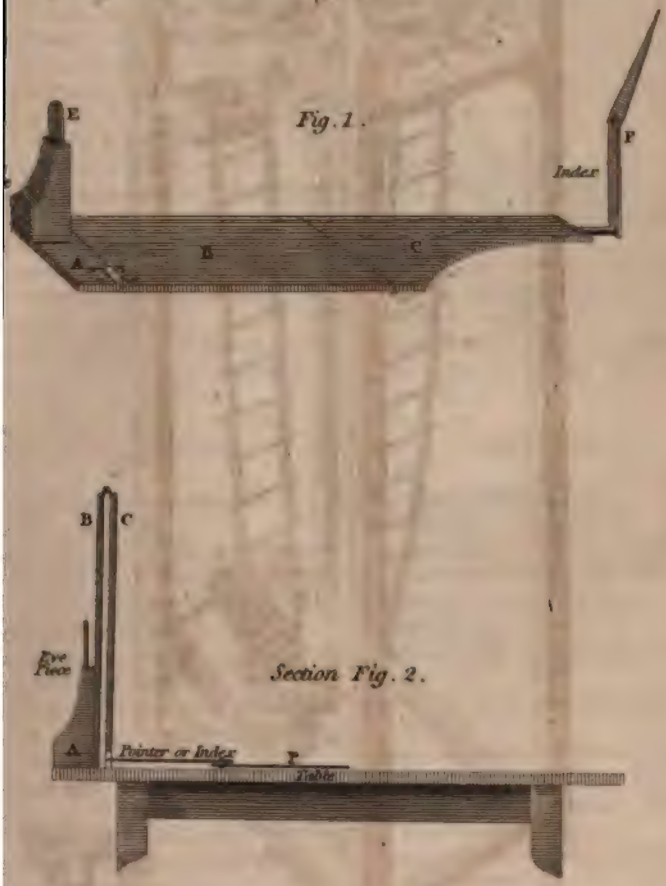
Machine for determining the positions of objects in drawing from Nature.



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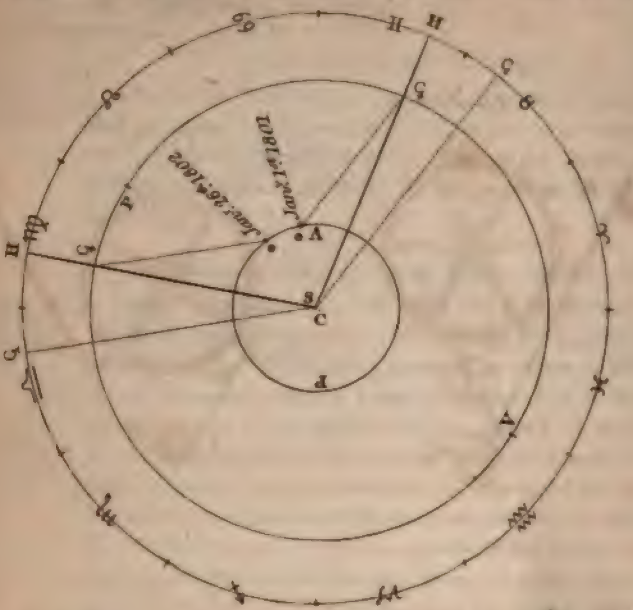

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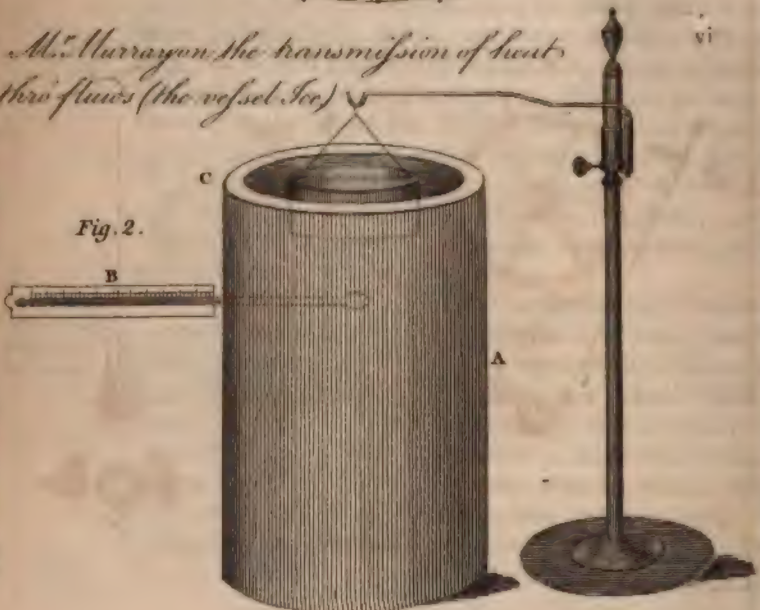

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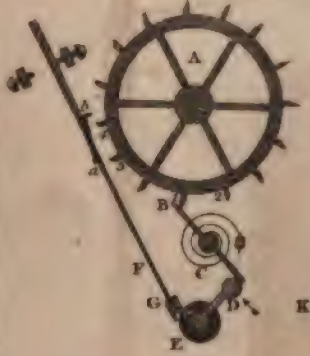

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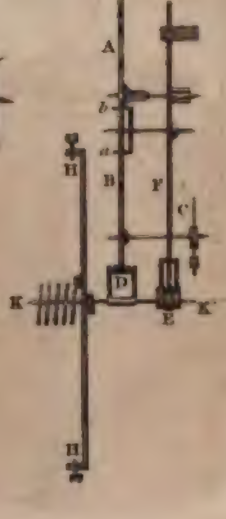

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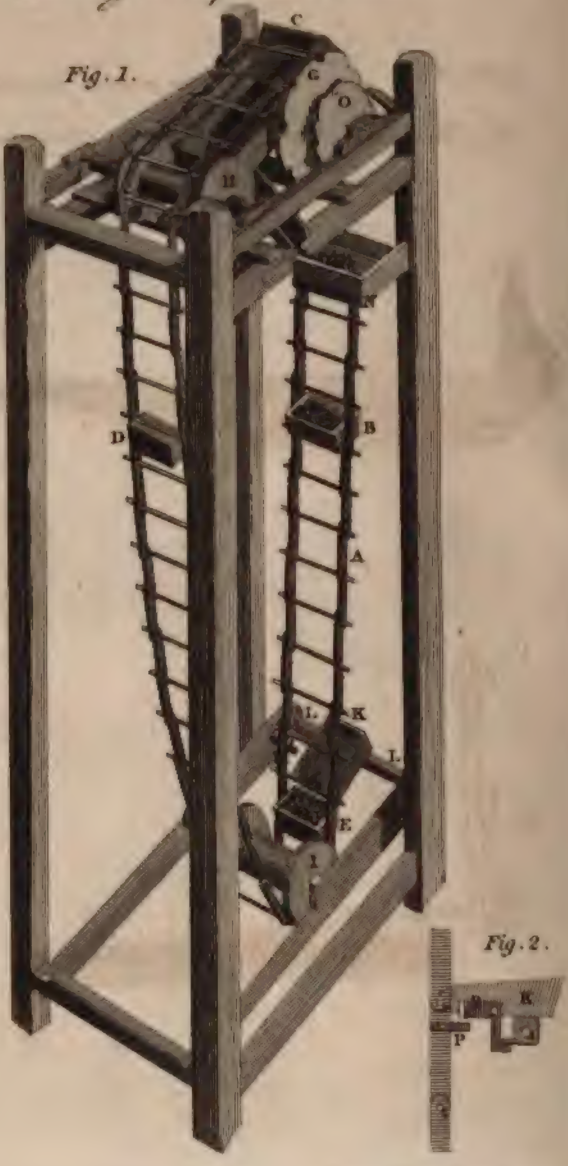

A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. II.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

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1802.

PREFACE.

I MEET my Readers and Correspondents at the completion of this Second Volume, with the satisfaction of directing their notice to the still greater increase of communication, and the means of rendering this work most eminently interesting and useful. It is not necessary that I should recapitulate the outlines of the plan and rules of conduct by which I have endeavoured to deserve the encouragement of the Public; but I think it truly justifiable to exhibit the marks of that approval at the same time that I express the sensibility with which I accept them, and the spirit they must afford to my labours.

Without entering into any estimate of the quantity of original matter in this work, which from the high value of its correſpondence is now become the authentic repository of the researches of our philosophers; I will only notice, that the number of original writers in this Volume is more than double that of the reſpectable list prefixed to the first Volume, at the same time that the value and importance of the Memoirs from foreign and domestic publications have continued to increaſe.

The authors of original Papers are Mr. F. Accum; A. B. C; John Bostock, M. D. J. W. Boswell; Count de Bournon; H. Campbell, M. D.; R. Chenevix, Esq.; Mr. John Clennel; C. P.; Mr. Wm. Close; W. Cruickshank, Prof. at Woolwich; John Cuthbertson; D. H.; J. Fletcher, Eſq. G. H; John Gough, Esq.; Mr. Olinthus Gregory; J. C. Hornblower; Rob. Jameson; J.; Rev. W. Pearson; N. N. Joseph Priestley, L.L.D. F.R.S. &c.; H. Sarjeant, Esq.; Mr. Tho. Sheldrake; Dr. J. H. Schroeter; Tho. Thomson, M.D.; Mr. Trevithick; Troughton; Ez. Walker; Rev. James Wilson, D.D. James Woodhouse, M.D.; Mr. Arthur Woolf; Thomas Young, M. D. Prof. P. R. I.; Baron von Zach; and W. N.—Of foreign works, Carcel; Carreau; Coulomb; Descroizilles; Guillot; Guyton; Hassel Lachenaie; Lalande; Pictet; Proust; Valentine; Vauquelin.—And of English Memoirs abridged or extracted, Mr. Banks; Wm. Bullock; Sir H. Englefield, Bart.; Mr. Gilpin; J. Gough, Esq.; Ch. Hatchett, Esq.; Wm. Herschel, L.L.D. Ed. Howard, Esq.; Hulme, M.D.; E. Jones, Esq. H. Sarjeant, Esq.

The novelty and excellence of the communications with *which this Journal* has been honoured, have been productive

tive of an effect which I have seriously meditated to remedy. In the limited extent of every work of this nature, when the new and interesting Memoirs demand a larger portion of its capacity, a smaller must of course be devoted to less striking, though doubtless very important business of selection. Foreign and domestic matter must be more fastidiously sorted out; articles must be abridged instead of being given at full length; and some must be rejected altogether that would have been highly acceptable, if the original productions could have allowed room.

Two remedies present themselves. The first is to print a supplementary number to each volume; and the other to give a greater number of pages without adding to the price. I shall be happy to adopt the latter as soon as the increased fale shall have rendered it practicable, without diminishing the ordinary remuneration the Work affords; and in case the former should prove necessary or advisable, I am confident my Readers will see the advantages and approve the proceeding. In the mean time, the private recommendation of men of merit to their friends, is the best means of accelerating that circulation which will eventually benefit its patrons, by the greater quantity of them that could in that case be afforded.

I conclude this Preface as usual, by mentioning the fubjects of the sixteen Plates which illustrate the present Volume. 1. Improvements in Hydraulic Engines, by Mr. Boswell. 2. Guyton's Improvements of the Swedish Stove. 3. Mr. Sarjeant's cheap Engine for raising Water. 4. Strong framed Levers for Steam Engines, by Mr. Hornblower. 5. Dr. Young's Diagrams to illustrate the Theory of Light. 6. Mr. Gregory's Figure for Mr. Pearfon's Analogy. 7. Mr. Gough's Illustration of the Doctrine of Sound. 8. Mechanical Lamp of Carcel and Carreau. 9. Mr. Woolf's Apparatus for heating Water by waste Steam. 10. Mr. Terry's improved Mill. 11. Mr. Bullock's Lock. 12. Count Bournon's Figures of Anhydrous Sulphate. 13. Mr. Trevithick's Application of a temporary Forcer. 14. Lachenaie's Apparatus for claying Sugars. 15. Mr. Jameson's Illustration of the Formation of Granite. 17. Mr. Banks's Instruments for determining the Pressures and Velocities of effluent Air or Gas. 18. The Spirit Hydrometer and Scales of Atkins. And, 19. Compound Condensers of Electricity, by *Mr. Read* and Mr. Cuthbertson.

Soho Square, London, September 1, 1802.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1802.

ARTICLE I.

Improvements in the Hydraulic Engine of SCHEMNITZ, *and that of Mr.* GOODWYN; *with comparative Remarks on the most useful Applications of each, and some Facts relative to the Invention of the pressure Engine. In a Letter from Mr.* JOHN WHITLEY BOSWELL.

To Mr. NICHOLSON.

SIR,

London, March 14. 1802.

HAVING, with much satisfaction, found that the method of making the Schemnitz hydraulic engine work itself, which I gave you for your excellent Journal in 1800, (IV. 117.) has been since found of considerable utility in * other works of a similar nature, I am induced to send for your approbation a draft of the † application of the same principle to Mr. Good- Method of working engines by the bucket and syphon.

* Vide Mr. Close's papers in the same Vol.

† The paper signed L in the quarto Journal, page 343, though it professes to shew how Mr. Goodwyn's engine may work itself, has only hinted at this method, but has not shewn how it may be effected.

wyn's engine, and another of a method of caufing the Schemnitz to raife water *above* the level of the prime refervoir, together with a comparative view of the advantages of both engines and their powers.

Concife explanation of the Schemnitz engine.

After a perufal of my former Paper on the Schemnitz engine, (IV. 144.) a mere infpection of the figure given here (Plate I.) will be fufficient to fhew the manner in which this now propofed will operate. The moving power is the preffure of the column of water from the refervoir R, (Fig. 1.) to D in the lower air chamber A, which forces the air contained in it into the chamber B, which air fo compreffed in B will impel the water contained in it upwards through the pipe to a height, and in a quantity proportionate to the relative height of the column of water contained in the pipe R D, compared with that contained in S B, or fuppofing the length of R D given, the greater the length of the pipe S B is, (fo as not to exceed R D,) the lefs will be the quantity of water delivered at S, and *vice verfa*.

Mr. Goodwyn's engine; conftructed for heavy work, and made to operate without attendance.

In the draft of Mr. Goodwyn's engine, Fig. 2. I have endeavoured to exhibit it as it fhould be if executed on a large fcale, and made all the pipes detached from each other, becaufe though the plan of making one pipe pafs through another and through the refervoirs, made ufe of in Mr. Goodwyn's model, is very convenient and neat in an apparatus that may be placed on a table, yet it would be found to produce an unneceffary trouble, complication, and difficulty of repair in a large engine. The method fhewn in this draft of caufing the engine to work without attendance, is the fame as for the Schemnitz, and caufes the cocks G and H to open at intervals, (which may be regulated at pleafure by the hand cock I, letting the water flow more or lefs quick into the fyphon veffel E,) while at the fame time it clofes the cock at D, and *vice verfa*. Self-moving valves are placed at the delivering pipes of the chambers C and B, and alfo at the air vent of A, becaufe wherever they can be ufed they are preferable to cocks, or valves ufed by external power ; fome doubt may arife, whether there fhould not be a paffage for the air let at intervals into A, as well as one for it to efcape : but as great quantities of air are contained in water, which the mode of working of this engine will particularly tend to feparate from it, I think it would be needlef and that the felf-moving valve

valve opening outwards at the air vent of A will be ſufficient. The pipe at K is to conduct water from the bucket F to that of the cock D. There are two ranges of reſervoirs repreſented, to ſhew the method of raiſing water by this engine about thirty feet high: more would be uſeleſs, and even a ſecond would in very few inſtances be found neceſſary, except when the fall of water from R to D was very ſhort, in which caſe it would be better to uſe ſome other engine for raiſing water to the required height.

In comparing thoſe two engines, it will be found that their powers and capabilities are nearly ſimilar. Compariſon of the powers of theſe two engines; numerically ſtated.

1. In both the greater the height of the original fall of water, denoted by the pipe R D, and the greater the quantity of water which it can ſupply in a given time, the greater quantity can be raiſed by theſe engines in a given time.

2. Both engines can be conſtructed ſo as to raiſe water above the original level, and from below to the ſurface, or from a pit.

3. By a ſucceſſive number of reſervoirs both engines can be brought to raiſe water to any height; but as they will raiſe a ſmaller quantity as the height is increaſed, the quantity wanted in a given time, and the expence of conſtruction, will limit the extent of their elevation.

4. In both engines the diſtance from one reſervoir to another, muſt always be leſs than that of the original fall R D. The circumſtances in which thoſe engines differ ariſe from the difference of their manner of action.

5. The Schemnitz engine operates by cauſing a fall of water to *compreſs* air, which re-acting on other water forces it to riſe in a pipe to a certain height. Mr. Goodwyn's engine acts by cauſing a fall of water to *rarify* a certain quantity of air, into whoſe ſpace the preſſure of the atmoſphere forces, when permitted, a quantity of water.

6. Hence in the Schemnitz engine, the preſſure acting from *within outwards*, tends to *burſt* the veſſels uſed in the ſtructure, and to *open* and *extend* any fiſſures which may chance to be in them.

7. In Mr. Goodwyn's engine the preſſure acting from *without inwards*, cloſes all the parts of which it is compoſed more together, tends to make its pipes and veſſels more ſtaunch, and in any fiſſure makes its ſides operate like valves to ſhut it up.

 8. The

8. The Schemnitz engine will always raife water to a height nearly equal to that of the original fall, from one refervoir to another, fuppofing the original fall of any height whatfoever, as 100 feet.

Mr. Goodwyn's engine will not raife water from one refervoir to another fo high as thirty feet in any cafe whatfoever, as there cannot be a complete vacuum formed by it in the air chamber, but only an approximation to one.

Mr. Goodwyn's engine will be preferable, and leaft coftly in fmall elevations.

From this comparifon it will follow, that wherever the original fall of water is lefs than thirty-two feet, Mr. Goodwyn's engine will be much preferable to the Schemnitz; as, from the 7th article of the comparifon, it may be made of the *cheapeft materials*, of ftrong *wooden cafks* and *wooden pipes*; whereas the Schemnitz engine, from the 6th article, muft be made of the ftrongeft, and of courfe moft coftly materials, of caft iron at leaft, and that of confiderable thicknefs.

The Schemnitz engine for greater depths.

But wherever the original fall exceeds the height of thirty feet much, and it is required to raife the water to nearly the fame height, then the Schemnitz engine appears to be preferable; as, in all probability, the fewer number of parts which it will, in this cafe, require in its conftruction, will more than compenfate for its coftly materials.

Pifton engine beft for very great depths.

When it is required to raife water to a height, much greater than that of the original fall, above the firft level, or from a greater depth; either from the original fall being fhort, or the required height being great, an engine in which the preffure of the water is made to act by a pifton in an apparatus fimilar to that of the fteam engine, (one of which is defcribed in your Journal for March) will be preferable to either of the above.

Comparifon of the three engines.

The comparifon of thofe engines may be brought to this one point: wherever Mr. Goodwyn's engine can be ufed, with a fingle continued pipe for elevating the water, and without a fucceffion of refervoirs, it feems to be the cheapeft.

Where Mr. Goodwyn's engine cannot be ufed without violating this condition, but the Schemnitz can, it promifes to be the next in point of cheapnefs, from its fimplicity, abfence of friction, and fmall number of working parts.

But when neither Mr. Goodwyn's engine nor the Schemnitz can be ufed without a number of refervoirs, then the pifton preffure engine probably ought to be preferred; but this will much depend on the number of refervoirs, for perhaps one or

two

two in addition to the Schemnitz might coft lefs, than boring the cylinder of the pifton engine perfect, and its additional machinery: for merely raifing water the powers of each are nearly equal, depending all on the height of the original fall of water.

The great advantage of the pifton preffure engine is, not as a cheap engine for raifing water, but as that in which a fall of water can be applied without any *wafte* to work mills or machinery for *any purpofe;* which is of very great confequence when the fall of water is of confiderable height, and the ftream, or fupply, fmall.

The preffure engine works *without wafte of water.*

Conceiving it of very great importance to have it determined in what fituations each of the principal engines, worked by water preffure, is to be preferred, I have commenced this comparifon, and if this fhall be acceptable, will fend another paper on a fimilar fubject; that is, on the comparifon of the common mill water wheel, with another mode of applying the water to turn mill work, which, I think, I can demonftrate to be much preferable. I hope what I have thus began will excite fome others to the fame enquiry; and that by this means the multiplicity of water preffure engines will be at laft arranged, and their comparative utility afcertained, fo that in every different cafe of fall or fupply of water, an engineer may know at once which he fhould ufe.

Great importance of comparing the different methods of applying water as a force.—Wheel work, &c.

I beg leave to add here fome remarks on the pifton preffure engine in your Journal for March: Mr. Trevithack will, I hope, excufe my taking from him the honour of his being the firft inventor of this mode of applying a fall of water, to give it back to Meffrs. Denifard and Deuille of France, when I confefs that for a long time I was in the fame error with him, and thought it had firft occurred to me, and propofed it with that idea to Mr. Carnac in Nov. 1796, to draw the water from his copper mine, (by which he would have faved the daily labour of twenty men, as he had a fall of water very proper for this engine—a fact which I have Mr. Carnac's fignature to prove;) but I fince found out that in Belidor's Hydraulic Architecture, publifhed at Paris 1739, in the fourth book and firft chapter, there is a method defcribed at large, with very well executed plates, by which a fall of water operating in a cylinder on a pifton may work a pump to force water to a greater height; and what is remarkable, Belidor

Mr. Trevithack's preffure engine was executed in 1731 by Denifard and Deuille—and imagined by Mr. Bofwell in 1796.

Defcribed in Belidor.

2 propofes

propofes the very fame method of working the valves by a tumbling weight, as that called the tumbling bob, in Mr. T's defcription. In Belidor's engine the pifton cylinder and pump are both horizontal, which is the moft material difference, but the principle is entirely the fame as Mr. Trevithack's.

Mr. Belidor does not claim the honour of this invention, but only propofes the engine I allude to, as an improvement on one executed on the fame principle at Seve, between Verfailles and Paris, in the year 1731, by Denifard and Deuille, for which they obtained a patent from the king of France for twenty-one years; of which engine there is alfo a defcription in the fame chapter of the above work.

Propofal of air chambers in the pifton.

I beg leave to fuggeft, that it might be a confiderable improvement to this engine to have its action made elaftic, by the addition of an air chamber, on the fame principle as that ufed in engines for extinguifhing conflagrations; fuch a one, feems to me, might be beft effected by making the pifton hollow, and of a larger fize, to ferve for this purpofe, as the air fpring would then act both on the upper and lower preffure of the water; Figure 3 is a fketch of this method, in which A reprefents the hollow pifton.

I hope the length of this paper will be excufed by the circumftance of my not having received your Journal for March till I had written the moft of it, and having had of courfe, to add the remarks on the pifton preffure engine to the reft.

I am, SIR, &c.

W. H. B.

II.

Remarks of the prefent State of Paper-making in England and France. By* H. CAMPBELL, *M. D. Communicated by the Author, March* 26, 1802.

Whether the additional duty on paper has principally injured book trade.

THE additional duty on Paper has not been the chief caufe of the diminution in the book and paper trades of this country. After

* The importance and authenticity of the chemical, mechanical, and other facts ftated in this Memoir, and of the object itfelf in general

After eftablifhing this fact, it will be equally evident that the paper and book trades will not be *effectually* relieved by giving up a part, or the whole of the laft additional duty.

The former adminiftration of this country by impofing an additional duty, did not teach the French the new art of paper making; nor did it fupply them with abundance of raw material; or fhew them the art of equalizing the different qualities of rags by Chaptal's mode of bleaching;—neither was any new light thrown by the additional duty upon the mode of printing and type cafting, as now practifed in France. France has fucceeded by improvements in the arts of paper making and printing.

Before the commencement of the late war, paper-making in this country, ftrictly fpeaking, was a mechanical art. The fuperiority of Englifh paper arofe from the fuperior linen worn by Englifh people. Their rags were fuperior to the rags collected on the continent. The decency of the Englifh populace, compared with the populace on the continent, could not be better fhewn than by an exhibition of Englifh and foreign rags. Rags called *London fines*, the refufe of Irifh linen, &c. could fcarce be equalled in any other country. The original linen of thefe rags had been highly bleached. They were confequently calculated to make the fineft and whiteft paper. Rags at an Englifh paper mill previous to the war, were *forted* according to their colour and finenefs. Pains were taken to cut off the feams and offal parts; and thefe parts were deftined to make inferior paper. This ftatement alone is almoft fufficient to give a clear notion of what I am about to eftablifh. The Britifh fine paper formerly excelled from the finenefs of our *linen rags*; and our careful forting them.

I conceive that a reprefentation of the *mechanical neceffaries*, as the dufter, the knives placed in the engine roller, the plate, the vat, the moulds, and laftly, flannels and preffes, although indifpenfible in a paper mill, are neceffary to be noticed in this account; as an imitation of them in France, could not contribute to the prefent alarming change in our paper trade. Our machinery.

The Englifh ftaple, fuperior rags, Englifh cleanlinefs in cutting and afforting, and better engines and knives, conftituted attempted to be transferred to France.

general to the progrefs of fcience and literature, leave no doubt refpecting the propriety of inferting it in our Journal, though its moft prominent feature relates to political regulation. On this laft fubject my conduct as a Journalift cannot be fuppofed to exprefs any opinion as an individual.---N.

tuted our fuperiority. About the year 1789 and 90, *certain refpectable* Englifh paper makers endeavoured to take thefe advantages to France. France apparently required them; her coarfe foul cotton rags were not made better by feparation and affortment; her engines were deficient, and her mills exhibited no mark of profperity, but every fymptom of flovenly neglect.

French rags are *coarfe cotton.*

French paper for copper plates.

One fort of paper, notwithftanding the want of colour and cleanlinefs, fhe excelled in, upon neceffity. I mean paper for copper plate prints; fhe neceffarily excelled becaufe her ftaple cotton rag being more bibulous, received better impreffions from the plate. This was the ftate of her paper trade and mills in the year 1789. Before that period, fhe did not confume all her own rags. We received a confiderable part from the cellars of Dunkirk and Oftend, and from countries in her fouthern vicinity, Leghorn, &c.

New æra in paper making. The bleaching procefs by ox. mur. acid.

About this time a new æra in paper making commenced. Chemiftry by her difciples, Scheele, Berthollet, and Chaptal, from a metallic oxide, folicited and directed the concentrated and pure part of the atmofphere, oxigen, to remove with expedition the colouring part of cloth, or rags made from vegetable fubftances, fuch as flax, cotton, hemp, &c. An attention to the beft bleaching procefs ought certainly to form a material part of the confiderations in this Paper; becaufe this object, connected with a knowledge of the forts of rags fuitable for bleaching, and making *printing* paper, would give that comprehenfion of the evil from which might be drawn —not fteps of temporary and unavailing expedience, but folid foundations of relief.

Coarfe cotton rags are brought to an equality with *fine linen* rags by that procefs.

The bufinefs under confideration is more intimately connected with printing than with writing paper. The bleaching gas is much better adapted to coarfe cotton rags than coarfe linen or hempen rags; becaufe the former is without ligneous particles, and the latter abounds with them, and thefe particles, called by paper makers *fheaves*, are made more confpicuous by bleaching. The ftaple of our opponents the French, confifts in coarfe cotton rags. I can, if required, point out places where depots fhould be eftablifhed for affording an unlimited fupply of fimilar *cheap* materials, and where each fhip, by way of finifhing her lading, will take on board a convenient number of bags.

A plentiful and cheap fupply of cotton rags.

In

In the year 1793-4, I imported a parcel of cotton rags at 9l. per ton; bleaching them added 8 per cent. to the 9l. The bleached ftuff was worth more than 40l. per ton: the beautiful paper produced now exifts in a public work. If paper makers, or ftationer-paper makers, had paid attention to this new and growing improvement, (or would pay attention) inftead of foliciting a remiffion of duties, they might be able with their *capital* and mills ready formed, to counteract the French, and contribute ftill more to the ftate, than is paid at prefent. A relaxation of part of the duties, cannot be a radical relief. To forego an object of finance without remedying a complaint, can only be a compliance with the government, to participate in misfortune with the petitioners.

Inftance in point. The true remedy, ftated to confift in the adoption of the improvements, and the proper employ of our capital and works.

The neceffity of abolifhing improper combinations and power among journeymen paper makers, and the injury done to paper mills by London ftationers *importing and regulating the price of rags*, and monopolizing mills, are evident directions to relieve the fuffering part of the trade, and how to obtain revenue from the wealthy part, by way of granting licences, I fhall be happy, if required, to communicate.

Combinations and monopolies. Hints at *regulations by the State.*

The arts, commerce, and navigation of this country, are juftly confidered by the French government, to be the finews of England. Chaptal, Berthollet, and other enlightened men are encouraged to affect us, and benefit their own country in thefe particulars: that they have not been unfuccefsful is manifefted by the ftate of the paper trade in France.

General obfervations. The arts, commerce, navigation.

The maxim of trade finding its level, is too fupine a maxim in the prefent ftate of difcoveries. Factories are altering, and markets muft vary. Goods formed by mere *mixture*, fuch as faline and other bodies, require neither capital, nor machinery. Many of thefe are in a ftate of great cultivation in France. In arts refting on capital, machinery, and aptitude of hand, we fhall long remain unrivalled.

Level of trade.

What manufactories can be eafily transferred; and what are the moft permanent in a wealthy country.

At prefent I fhall check thefe obfervations, that my remarks on the paper trade may remain diftinct and unmixed with any other matter.

H. CAMPBELL.

No. 11, Fleet-Street, London.

III. *Remarks*

III.

Remarks on Combuſtion, by THOMAS THOMSON, *M. D. Lecturer on Chemiſtry in Edinburgh.*

Phenomenon of combuſtion is very ſtriking to every claſs of men.

NO operation of nature has a better claim to our attention than COMBUSTION. The irreſiſtible devaſtation which it ſometimes occaſions is calculated to ſtrike the ignorant with terror; the extraordinary changes which it produces naturally attract the inquiſitive eye of the philoſopher, while its ſubſerviency to almoſt every branch of domeſtic economy renders it a familiar and neceſſary agent in the hands of every individual. This familiar acquaintance with combuſtion ſeems, however, to have retarded the inveſtigation of its nature; for it was not till the ſeventeenth century that philoſophers made it a ſerious object of enquiry. The labours of Bacon, Boyle, Hooke, and Mayow are well known; and the ſucceſs with which theſe labours were attended, muſt, if we recollect the difficulties to be overcome, give us a very high idea of the genius of theſe inveſtigators of nature. But the philoſophers of our own age, eſpecially Lavoiſier, have gone far beyond their predeceſſors; and have explained ſome of the moſt intricate and important phenomena of combuſtion.

Eminent inveſtigators of its theory.

Lavoiſier's theory

Mr. Lavoiſier's theory of *combuſtion*, improperly termed his theory of *chemiſtry*, is ſo generally known, that it is unneceſſary to enter into any detail concerning it. Its merit is indiſputable, and raiſes its author to the very firſt rank among philoſophers. Many chemiſts ſeem to think that it explains the whole phenomena of combuſtion; but an attentive examination muſt convince every impartial obſerver, that the theory of Lavoiſier, ingenious and ſatisfactory without doubt, as far as it goes, leaves yet ſeveral parts of that very complicated proceſs as unaccountable as ever. He has corrected the errors of his predeceſſors, and made a very important new ſtep; but many new ſteps are ſtill wanting to render the theory complete. I hope, therefore, that the following remarks will not be conſidered as altogether improper; they will at leaſt exhibit the ſubject in a new point of view, and may perhaps contribute to call the attention of chemiſts to certain phenomena

leaves much to be ſtill elucidated.

phenomena which have not hitherto been claſſified, nor examined with that preciſion to which they are entitled.

1. Though the French chemiſts have lately given the term *combuſtion* a new meaning, and made it ſtand for the general *combination of a body with oxigen*, I mean, for reaſons which will appear hereafter, to employ it in the ſenſe uſually affixed to the term by the generality of mankind. Now when a body undergoes combuſtion, in the common ſenſe of the word, two things always take place. 1. The body gradually waſtes away, and often diſappears altogether; it is then ſaid to be *conſumed* or *burnt*. 2. During the whole of this proceſs it emits *heat* and *light*; the heat and light thus emitted are uſually denominated *fire*, and the waſte of the body is conſidered as the effect or conſequence of its combuſtion. If either of theſe two phenomena be wanting, we do not ſay in common language that a body is undergoing combuſtion, or that it is burning. Every theory of combuſtion then muſt explain, 1. Why the burning body is waſted and altered. 2. Why during the progreſs of this alteration heat and light are emitted.

The French confine the term combuſtion to the act of oxigenation.

Uſual acceptation of the term preferred, *conſumption or waſte with heat and light.*

The theory muſt explain theſe effects.

2. If we take a view of the different bodies which occupy the attention of chemiſts, we ſhall find, that as far as combuſtion is concerned, they may be arranged under three claſſes; namely, 1. Combuſtibles. 2. Supporters of combuſtion. 3. Incombuſtibles.

Relative to combuſtion bodies are 1. combuſtibles, or 2. ſupporters of combuſtion, or 3. incombuſtible.

I. The COMBUSTIBLES are thoſe bodies, which are ſaid in common language to *burn*. During combuſtion they appear to emit light and heat, and at the ſame time gradually waſte away. When this change has reached its maximum, the proceſs of combuſtion is at an end. The claſs of combuſtibles is very numerous; but all the bodies belonging to it may be ſubdivided into three ſets; namely,

I. *Combuſtibles* or the bodies conſumed.

1. Simple combuſtibles,
2. Compound combuſtibles,
3. Combuſtible oxides.

The *ſimple combuſtibles* are twenty-four or twenty-five in number, namely,

They are ſimple combuſtibles; or

1. Sulphur, 3. Carbon,
2. Phoſphorus, 4. Hidrogen gas,
5. All the metals *.

* Except perhaps gold, ſilver, and mercury.

The

—compound combuftibles,

The *compound combuftibles* confift of compounds formed by the fimple combuftibles uniting together two and two; and are of courfe much more numerous than the fimple combuftibles. They may be arranged under the five following heads:

1. Sulphurets, 3. Carburets,
2. Phofphurets, 4. Alloys,
5. Sulphurated, phofphorated, and carbonated hidrogen.

or combuftible oxides,

The *combuftible oxides* are compofed of one or more fimple combuftibles, combined with a dofe of oxigen. Though the French chemifts have given to thefe bodies the name of *oxides*, we fhall fee afterwards that they differ effentially from *metallic oxides* and from water, which is confidered at prefent as an oxide of hidrogen. The combuftible oxides may be arranged under two heads: 1. Thofe which contain only a fingle bafe combined with oxigen, and which therefore may be termed *fimple combuftible oxides*. 2. Thofe which contain more than one bafe combined with oxigen, and which therefore may be termed *compound combuftible oxides*.

and thefe laft are either fimple, having a fingle bafe, or compound, having more than one bafe.

Simple comb. oxides.

The fimple combuftible oxides are only four in number; namely,

1. Oxide of fulphur, 3. Charcoal,
2. Oxide of phofphorus, 4. Carbonic oxide gas.

Unlefs fulphur, phofphorus, and hidrogen gas, bodies at prefent confidered as fimple, belong to this clafs. All the fimple combuftible oxides are by combuftion converted into acids.

Compound comb. oxides.

The compound combuftible oxides include by far the greater number of combuftible bodies; for almoft all the animal and vegetable fubftances belonging to them. The double bafe is ufually carbon and hidrogen: alcohol, ether, refins, gums, &c. are inftances of compound combuftible oxides *.

Phlogifton.

It was believed by Stahl and his difciples, that all combuftible bodies contain one common principle, to which they owe their combuftibility. But in confequence of the difcoveries of Lavoifier this theory has been laid afide.

II. *Supporters of* combuftion

II. The *fupporters of combuftion* are a fet of bodies which are not of themfelves, ftrictly fpeaking, capable of undergoing combuftion, but which are abfolutely neceffary for the procefs; for no combuftible body can be made to burn unlefs

* To this clafs of bodies alfo muft be referred all the vegetable and animal acids.

fome

fome one or other of the *fupporters* be prefent. Whenever they are excluded the procefs ftops. All the fupporters known at prefent are the following fix:

1. Oxigen gas,	4. Nitrous gas *,
2. Air,	5. Nitric acid,
3. Gafeous oxide of azote,	6. Oximuriatic acid.

There are indeed certain fubftances befides thefe, which poffefs nearly the fame properties; thefe I fhall enumerate afterwards under the title of *partial fupporters*.

All the fupporters contain one common principle, namely, *oxigen*. The firft of them confifts of oxigen uncombined with any bafe; but in the other five the oxigen is united to a bafe. It is very remarkable, that in four cafes out of five, the bafe to which the oxigen in thefe compound fupporters is united is *azote*. Is it not probable from analogy, that oximuriatic acid, the remaining compound fupporter, contains azote likewife as a component part. universally contain oxigen. N. B.

III. The *incombuftible bodies* are neither capable of undergoing combuftion themfelves, nor of fupporting the combuftion of thofe bodies that are; of courfe they are not immediately connected with combuftion. At prefent we are acquainted with about 13 incombuftible bodies, not reckoning the compounds which they are capable of forming with each other. Thefe are, III. *Incombuftibles.*

1. Azotic gas,	3. The earths.
2. The fixed alkalies,	

The firft of thefe fubftances conftitutes the bafe of almoft all the compound fupporters. Some of the alkalies and earths poffefs certain properties in common with combuftibles, and are capable of exhibiting phenomena fomewhat analogous to combuftion; phenomena to be defcribed afterwards under the title of *femi-combuftion*. Semi-combuftion.

3. From the preceding obfervations it is obvious, that in every cafe of combuftion there muft be prefent a *combuftible* and a *fupporter*. Now during combuftion the combuftible always unites with the oxigen of the fupporter. It is this combination which occafions the apparent wafte and alteration of the combuftible. The new compound thus formed I fhall call a *product of combuftion*. Now every product of combuftion is either, Combuftion requires a *combuftible* and a *fupporter*. Product of combuftion is either *water* or an *acid*, or a *metallic oxide*.

* Mr. Davy firft proved that this gas is a fupporter.

either, 1. *water*, or 2. *an acid*, or 3. *a metallic oxide*. It is true indeed, that other bodies ſometimes make their appearance during combuſtion, but theſe will be found upon examination not to be products, nor to have undergone combuſtion.

Thus one of the two characteriſtic marks which diſtinguiſh combuſtion, namely, the apparent waſte and alteration of the combuſtible body, has been fully explained. For the explanation of it we are indebted to Lavoiſier. It conſtitutes what is uſually, but abſurdly, termed the *new theory of chemiſtry*, and is the moſt important ſtep which has been made towards a complete theory of combuſtion,

Facility of combuſtion is not proportioned to the attraction for oxigen.

But though the combination of the combuſtible with oxigen be a conſtant part of combuſtion, yet the facility with which combuſtibles burn is not proportional to their apparent affinity for oxigen. Phoſphorus, for inſtance, burns more readily than charcoal; yet charcoal is capable of abſtracting oxigen from phoſphorus, and of courſe has a greater affinity for it. The combuſtible oxides take fire more readily than ſome of the ſimple combuſtibles; thus charcoal burns more eaſily than carbon or diamond: alcohol, ether, and oils, are exceedingly combuſtible, whereas all the metals require a very high temperature when the ſupporter is air. This greater combuſtibility of combuſtible oxides is probably owing to the weaker affinity by which their particles are united. For the cohesion of heterogeneous particles, when oxigen conſtitutes a part of them, is uſually weaker than the cohesion of homogeneous particles. Hence they are more eaſily ſeparated than homogeneous particles, and of courſe combine more readily with oxigen; thoſe ſimple combuſtibles which melt eaſily, or which are in the ſtate of elaſtic fluids, are alſo very combuſtible, becauſe the coheſion between their particles is eaſily overcome.

But chiefly depends on the facility of deſtroying the coheſion.

Hence *compound ſupporters* are more readily burned.

It is owing to the ſame inferiority in the coheſion of heterogeneous particles, that ſome of the compound ſupporters occaſion combuſtion in circumſtances when the combuſtibles would not be acted on by ſimple ſupporters. Thus phoſphorus burns in air at the common temperature; but it does not burn in oxigen gas, unleſs the temperature exceed 90°. In oximuriatic acid gas phoſphorus burns rapidly at the common temperature of the air, and ſo do ſeveral of the metals;

 though

though they cannot be made to burn in air except at a very high temperature. Thus alſo oils burn rapidly when mixed with nitrous acid. Nitrous gas and the gaſeous oxide of azote conſtitute exceptions to this rule.

Products of combuſtion are never combuſtible.

4. None of the *products* of combuſtion are combuſtible according to the definition of combuſtion which I have given. This want of combuſtibility is not owing to their being ſaturated with oxigen; for ſeveral of them are capable of combining with an additional doſe of it. But during this combination no caloric nor light is ever emitted; and the compound formed differs eſſentially from a *product* of combuſtion; for by this additional doſe of oxigen the *product* is converted into a *ſupporter*.

Oxigenation of a product converts it into a ſupporter.

Difference between combuſtion and oxigenation.

Hence we ſee that combuſtion ought not to be confounded with the combination of a body with oxigen, as is done by the French chemiſts. Combuſtion indeed cannot take place without the combination of oxigen; but oxigen may combine without combuſtion. Thus when iron is burnt, it always combines with 0.27 of oxigen, and is converted into the *black oxide*, a product of combuſtion, and altogether incombuſtible; capable, however, of combining with an additional doſe of oxigen, and of being converted into the *red oxide*. But during this laſt combination, how rapidly ſoever it takes place, no heat nor light is emitted. Now the *red oxide of iron* is not a product of combuſtion, but a ſupporter; as the following experiments demonſtrate: Mix it with phoſphorous, and put the mixture into the bottom of a long glaſs tube, ſhut at one end, and filled with azotic gas. Cloſe the mouth of the tube, and apply heat to that part in which the mixture is. At a certain temperature a violent detonation takes place, which ſhatters the tube in pieces. It is needleſs to remark, that the tube muſt be ſufficiently long to prevent the effects of expanſion in the gas included.

Detonation of phoſphorus with oxide of iron.

Other products converted into ſupporters.

When antimony is burnt, it always combines with 0.20 of oxigen, and is converted into the *white oxide*. Now this white oxide, which is a product of combuſtion, and of courſe incombuſtible, is capable of combining with an additional doſe of oxigen, and of being converted into the *acidulous oxide* of antimony. In like manner, lead, when burnt, is converted into the *white oxide* of lead, a product; but this product combines with

with additional doses of oxigen, and is converted into the *red* and *brown* oxides, both of which are supporters. When the *supporters*, thus formed by the combination of oxigen with *products*, are made to support combustion, they do not lose all their oxigen, but only the additional dose which constituted them supporters. Of course they are again reduced to their original state of products of combustion. Hence it follows, that they owe their properties as supporters, not to the whole of the oxigen which they contain, but to the additional dose which constituted them supporters. We may therefore call them *partial supporters*, indicating by the term, that part only of their oxigen is capable of supporting combustion, and not the whole. It is very possible that both *azote* and *muriatic acid* may be products of combustion; and in that case both the compound and partial supporters would agree with each other in every respect. In the present state of our knowledge, however, it is necessary to distinguish them.

They act by part of their oxigen; and are *partial supporters*.

The bases of all known *partial supporters* are metallic.

All the partial supporters with which we are acquainted, contain a metallic basis; for metallic oxides are the only products at present known capable of combining with an additional dose of oxigen. It is a circumstance highly deserving of attention, that when metals are capable of combining with several doses of oxigen, the product or oxide formed by combustion is seldom or never that which contains a maximum of oxigen. The following oxides are products of combustion:

Enumeration.

1. Black oxide of iron.	8. Oxide of copper *.
2. White oxide of zinc.	9. Oxide of cobalt *.
3. White oxide of lead.	10. Oxide of nickel *.
4. Yellow oxide of tin.	11. Oxide of bismuth *.
5. White oxide of antimony.	12. Purple oxide of gold?
6. White oxide of arsenic.	13. Yellow oxide of silver?
7. White oxide of manganese.	14. Black oxide of mercury †?

* The particular oxide of these metals, which is the product of combustion, has not been ascertained; but they are all combustible in oximuriatic acid gas.

† I doubt much whether gold, silver, and mercury, be combustible at all. They do not burn in air, how high soever the temperature is; neither do they detonate with red hot nitre, nor exhibit any appearance of combustion in oximuriatic acid gas; though this last body oxidates them with great rapidity.

The

The following oxides, on the other hand, are partial supporters of combuftion:

1. Red oxide of iron *.	6. Red and brown oxides of lead **.
2. Yellow oxide of gold †.	7. Black oxide of manganefe.
3. White oxide of filver ‡.	8. Acidulous oxide of antimony?
4. Red oxide of mercury §.	9. White oxide of tin ?
5. Arfenic acid ‖.	

This lift would doubtlefs be increafed by an accurate examination of all the metallic oxides not included in either of thefe tables.

Thus it appears that feveral of the products of combuftion are capable of combining with oxigen. The incombuftibility of products, therefore, is not owing to their want of affinity for oxigen, but to fome other caufe. Incombuftibility of *products* is not owing to want of affinity for oxigen.

5. No *product* of combuftion is capable of *fupporting* combuftion. This is not occafioned by any want of affinity for combuftible bodies; for feveral of them are capable of combining with an additional dofe of their bafis. But by this combination they lofe their properties as products, and are converted into *combuftibles*. The procefs therefore differs effentially from combuftion. Thus fulphuric acid, a *product* of combuftion, by combining with an additional dofe of fulphur or its oxide, is converted into *fulphureous acid*, a fubftance which, from feveral of its properties, I conclude to be combuftible. Thus alfo phofphoric acid, a product of combuftion, is capable of combining with phofphorated hidrogen, and of forming *phofphorous* acid a combuftible body. When this laft acid is heated in contact with a fupporter, it undergoes combuftion; but it is only the additional dofe of the combuftible which burns, and the whole is converted into phofphoric acid. Hence we fee that it is not the whole bafis of thefe compounds which is combuftible, but merely the additional dofe. The compounds, therefore, formed by the union of a product and combuftible,

Nor can they *fupport* combuftion, though they can combine with combuftibles.

Inftances. This combination affords *partial combuftibles*, acting by part of their bafis.

* Fulminates with phofphorus.
† Forms fulminating gold.
‡ Forms fulminating filver.
§ Forms fulminating mercury.
‖ Occafions combuftion when heated with feveral combuftibles.
** Occafions the combuftion of fulphur.

may be termed *partial combuftibles*; indicating by the name, that a part only of the bafe is capable of undergoing combuftion. Now fince the products of combuftion are capable of combining with oxigen, but never exhibit the phenomena of combuftion except when they are in the ftate of partial combuftibles, combuftible bodies muft contain fome principle which they lofe during combuftion, and to which they owe their combuftibility; for after they have loft it, they unite to oxigen without exhibiting the phenomena of combuftion.

Since *products* can combine with oxigen, but never exhibit combuftion unlefs they be *partial combuftibles*, combuftibles muft contain a fubftance which they lofe in burning.

Though the products of combuftion are not capable of fupporting combuftion, they not unfrequently part with their oxigen juft as fupporters do, give it out to combuftibles, and convert them into products; but during this procefs no heat nor light is ever evolved. Water, for inftance, gives out its oxigen to iron, and converts it into *black oxide*, a product; and fulphuric acid gives out its oxigen to phofphorus, and converts it into phofphoric acid. Thus we fee that the oxigen of products is capable of converting combuftibles into products, juft as the oxigen of fupporters; but during the combination of the laft only, are heat and light emitted. The oxigen of fupporters then contains fomething which the oxigen of products wants.

Products can give oxigen to *combuftibles*, and convert them into products; but they do not, like *fupporters*, caufe combuftion.

The oxigen of *fupporters* differs from that of *products*.

6. Whenever the whole of the oxigen is abftracted from products, the combuftibility of their bafe is reftored as completely as before combuftion; but no fubftance is capable of abftracting the whole of the oxigen, except a *combuftible* or a *partial combuftible*. Water, for inftance, is a product of combuftion, whofe bafe is hidrogen. To reftore the combuftibility of the hidrogen, we have only to mix water with iron or zinc filings; the metal is oxidated, and the hidrogen gas is evolved as combuftible as ever. But no fubftance, except a combuftible, is capable of feparating hidrogen gas from water by combining with its oxigen. In the fame manner phofphorus abforbs the oxigen from fulphuric acid, and is converted into a product, while fulphur is feparated in its ufual ftate of combuftibility. Thus we fee that combuftibles are capable of reftoring the combuftibility of the bafes of products, but they themfelves lofe their combuftibility by the procefs, and are converted into products. Combuftibility, therefore, may be thrown at pleafure from one body to another. This fact was firft fet in a proper light by Stahl, and was the great ftep in the theory of combuftion

Combuftibility is reftored to *products* only by *combuftibles*.

Doctrine of Stahl.

combuftion for which we are indebted to that philofopher. Some miftakes into which he fell were afterwards corrected by Lavoifier *.

From thefe facts it is obvious, that the products of combuftion may be formed without combuftion; but in thefe cafes a new combuftible is always evolved. The procefs is merely an interchange of combuftibility; for the combuftible is converted into a product only by means of a product. Both the oxigen and the bafe of the product having undergone combuftion, have loft fomething which is effential to combuftion. The procefs is merely a double decompofition. The product yields its oxigen to the combuftible, while at the fame time the combuftible gives out fomething to the bafe of the product; the combuftibility of that bafe then is reftored by the lofs of its oxigen, and by the reftoration of fomething which it receives from the other combuftible thus converted into a product.

When products are thus formed without combuftion, a new combuftible is evolved.

There is indeed another method of forming the products of combuftion without actual combuftion in certain cafes; but the phenomena are much more complicated. This method is to expofe them to the action of fome of the fupporters diffolved in water; efpecially nitric acid. Thus moft of the metallic oxides may be formed without combuftion by the action of that acid on the metals. But in that cafe a new fupporter is always evolved, namely, nitrous gas; ammonia, a new combuftible, is ufually alfo formed; and not unfrequently the *product* is converted into a *partial fupporter*.

Complicated cafe of products formed by nitric acid, &c.

7. No *fupporter* can be produced by combuftion, or by any equivalent procefs. Now as all the fupporters, except oxigen gas, confift of oxigen combined with a bafe, it follows as a confequence, that oxigen may combine with a bafe without lofing that ingredient, whatever it is, which gives occafion to combuftion. The act of combination of oxigen with a bafe, therefore, is by no means the fame with combuftion. If we

Oxigen may combine without lofing the ingredient which occafions combuftion.

* When fulphate of iron is dropt into a folution of muriate of gold, nitrate of filver, or nitrate of mercury, the gold, filver, and mercury are precipitated in the metallic ftate. This is an additional reafon for fufpecting that thefe three metals are not combuftible. Every perfon, however, muft have obferved, that the metals in queftion have not the metallic luftre when firft precipitated, and that they acquire it flowly when allowed to remain expofed to the light.

All supporters procured either from other supporters, or by electricity.

taken view of the different fupporters, we fhall find that all of them which can be obtained artificially, are procured either from other fupporters, or by the agency of electricity.

Oxigen.

I. OXIGEN GAS may be procured from nitric acid and oximuriatic acid, two fupporters; and from feveral of the partial fupporters, as the black oxide of manganefe, the red oxides of lead and of mercury. The action of heat is always neceffary; but the procefs is very different from combuftion.

Air.

II. AIR, as far as is known at prefent, cannot be formed artificially. The gas indeed which comes over during part of the ufual diftillation of nitre and fulphuric acid to obtain nitrous acid, refembles air very clofely. But it is obtained from a fupporter.

Oxide of azote.

III. The GASEOUS OXIDE of azote, or nitrous oxide of Davy, has hitherto been only procured from nitrous gas and nitric acid (nitrate of ammonia), both of which are fupporters.

Nitrous gas.

IV. NITROUS GAS can only be procured by the decompofition of nitric acid, a fupporter.

Oximuriatic acid.

V. OXIMURIATIC ACID can be formed by combining muriatic acid with the oxigen of nitric acid, a fupporter; or with the oxigen of the black oxide of manganefe, the red oxides of lead, iron, mercury, &c. all of which are partial fupporters.

Nitric acid formed fpontaneoufly.

VI. NITRIC ACID is formed fpontaneoufly upon the furface of the earth by proceffes with which we are unacquainted.

(To be continued.)

IV.

Some Account of a new Planetary Body, difcovered by DR. OLBERS, *on the 28th of March,* 1802.

Difcovery of a new planet.

A LETTER from Dr. J. H. Schroeter, of Lilienthal, to Mr. Beft, London, was lately read at the Royal Society, in which he gives an account of this remarkable difcovery of a fecond planetary body of fmall apparent magnitude, difcovered by Dr. Olbers, of Bremen, on the 28th of laft March, when it formed the fouth point of an equilateral triangle with the ftars Nos. 20 and 19 of the conftellation Virgo. During the comparifons he was induced to make, from his conviction that it was not there in January when he re-difcovered the Ceres near

near the fame fpot, he found its change of place to be very perceptible. The following obfervations were tranfmitted by him to Dr. Schroeter:

	d.	h.	m.	s.						
1802. Mar.	28	9	25	10 M. T.	App. R. Af.	184°	56′	49″		Its place.
					App. Dec.	11	33	30 N.		
	29	8	49	14 M. T.	App. R. Af.	184	46	36		
					App. Dec.	11	52	59 N.		

Dr. Olbers had not the means of obferving any difc, but Dr. Schroeter, from his information, began to obferve it on the 30th with his thirteen feet reflector, while his affiftant, Mr. Harding, determined its place. With a power of 288 of this large inftrument, it appeared round and lefs hazy than Ceres, with a diameter of 4.635 feconds, which is much larger than thofe of Ceres and the Georgium Sidus; the former of which, on the 28th March, meafured 4.021 fec. and the latter, on the 20th, meafured 3.973 feconds. Its light, though pale and white in comparifon with that of Ceres, was neverthelefs more intenfe upon the whole, as appeared by its projection on the difc micrometer; but the Georgium Sidus was much brighter.

Its apparent diameter larger than that of Ceres or the Geo. Sidus.

Its light.

A minute ftar was feen near it on two feveral days, which Dr. Schroeter feems to fufpect as a fatellite.

Sufpicion of a fatellite.

The pofition of the Olberian planet (or comet), at March 30 d. 8 h. 20 m. 50 fec. mean time, was

App. R. A. 184° 35′ 52″ and 12° 15′ 8″ app. N. declin.

On the 1ft of April it was again obferved with the great reflector, and appeared to exhibit no difc, but was lefs in a brighter light with the power of 288, and could not be diftinguifhed from a fixed ftar. When it fometimes appeared with a difc, its diameter was only 3″.244. The Doctor is difpofed to afcribe this to the heavy dew.

No difc on April 1.

Letter from Mr. W. WALKER, *Lecturer on the Eidouranion.*

To Mr. NICHOLSON.

41, *Conduit Street, Hanover Square, London,*
April 26, 1802.

SIR,

THE account you did me the favour of inferting in your valuable Journal of the laft month, refpecting the fituation of the planet Ceres, will be rendered additionally interefting at prefent, from the circumftance of a ftill more recently obferved

moving ſtar being ſituated very near to the ſituation there pointed out as the place of the Ceres on the 25th of March. The planet or comet diſcovered by Dr. Olbers, at Bremen, on the 28th ult. is at preſent in a very ſmall degree to the S. E. of the ſituation of the Ceres on the 25th of March, and will readily be ſeen by a night-glaſs or teleſcope. It is inviſible to my naked eye, but appears of a definable diſc with a magnifying power of a 100 times. Its light is pale red and very faint, and through the night-glaſs is leſs brilliant than the Ceres, although no magnifying power that I can uſe will give the latter any apparent diameter. It ſeems probable that its diſtance is about as far again as the Earth is from the Sun, whilſt the Ceres is near three times the diſtance, and Mars about once and a half as far off. As my object is merely to enable any perſon to find it, I do not trouble you with any more full account at this late period of the month.

Preſent ſituation, April 26.

Definable diſc with power of 100. Pale red and leſs brilliant than Ceres. Suppoſed to be nearer the Earth.

I remain, SIR,
With much reſpect,
Your conſtant reader,
W. WALKER.

Preſent ſituation of Ceres.

The Ceres Ferdinandia will be found a little to the north-eaſt of the ſtar Beta in the Lion's Tail, being the eaſtermoſt point of a right angled triangle formed by Beta, a double ſtar due north of it and itſelf.

V.

On Bradley's Method of obſerving Tranſits, and another Method by which the Thickneſs of the Wire is rendered of no Importance. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Dr. Bradley's method of making tranſit obſervations deſcribed.

THE method of taking tranſit obſervations introduced by Dr. Bradley is ſtill uſed by aſtronomers. This conſiſts in noting the proportional diſtance of the ſtar from the wire at the two beats of the clock, one immediately preceding, and the other immediately following its paſſage acroſs the wire. If the wire be ſo thick as to cover the ſtar, and the ſtar happens to be behind it when the clock beats, the ſituation of the ſtar's

ſtar's centre cannot be exactly known, which makes the introduction of fine wires very deſirable. The fineſt wires ſtill proving too thick for very ſmall ſtars, an aſtronomical friend of mine hinted to Mr. Troughton that he might probably receive aſſiſtance in this delicate branch of his buſineſs from ſome of his ſpiders. This hint was not loſt,*—and hence Bradley's method of obſerving ſeems to be carried to the higheſt degree of perfection by the aſſiſtance of a harmleſs inſect which is perſecuted without mercy by every *fille de chambre* throughout his Majeſty's dominions.

Spider's webs in the foci of teleſcopes.

There is, however, another method of obſerving which precludes the neceſſity of very fine wires. This conſiſts in noting the time when the centre of the ſtar comes to the SIDE of the wire. But before this method is uſed it is neceſſary that one ſide of the middle wire ſhould be brought into the meridian; ſuppoſe it be that ſide which appears to the weſt when the teleſcope is turned to the ſouth, then the obſervations are to be taken on that ſide of all the wires.

Another method of obſerving one ſide of the wire.

It is a *line drawn by the ſtrength of imagination* down the middle of the wire, parallel to the ſides, which is uſed in Bradley's method, but in my method a *real line* is preſented to the eye of the obſerver and which he ſees very diſtinctly, although as fine as if it had been drawn through a geometrical definition.

Which is a real line.

This method of obſerving ſeems to be more ſimple than the other, in conſequence of its being, in many caſes only neceſſary to attend to one ſide of the wire: for example: Should the clock beat when half the ſtar is covered by the wire, it is evident that the centre of the ſtar paſſes the ſide of the wire at that time. And by making uſe of the apparent diameter of the ſtar, and the thickneſs of the wire as two meaſures, the fractional part of a ſecond may be eſtimated, by an experienced obſerver to a very great degree of preciſion.

Its advantages.

How far this ſmall alteration in the method of uſing the tranſit teleſcope may be found convenient to others experience muſt determine; but for my own part, I am certain that I can obſerve, not only with more eaſe to myſelf, but with greater exactneſs by this method than by Bradley's.

I am, Sir,

Your very humble ſervant,

Lynn, April 19, 1802. EZEKIEL WALKER.

* See a further account of this invention in the laſt vol. of this Journal, pa. 319.

VI. *Deſcription*

VI.

Description of a Stove on the Principles of the Swedish Fire-place, with Heat-openings, by CITIZEN GUYTON.*

Fire-places in France not generally constructed on good principles.

THE true principles of constructing fire-places, so as to obtain the greatest heat with the least consumption of fuel, have been known for some time in France; but they have been much less generally adopted, than the necessity for economising fuel demands. We see many fire-places so deep as to consume double the quantity of fuel necessary, and yet heat the apartment but faintly, where half the expence might be spared by altering the fire-place according to count Rumford's plan.

Often too deep.

Smoky chimneys how usually remedied.

If a chimney smoke, instead of reducing the tunnel to proper dimensions, so that descending currents cannot take place in it, scarcely any remedy is thought of but air-holes, which require the sacrifice of a certain quantity of fuel, to counterbalance the effect of the cold air continually entering.

The use of the Swedish stoves is probably yet rare, from their not having been constructed on just principles, or in the best proportions, at their first introduction. As I have had one made, which appears to many of my friends to produce an astonishing effect, in compliance with their request I shall give an exact description of it, premising however a few principles with regard to fires.

General principles with regard to fires for domestic purposes.

1. The heat produced is proportionate only to the air consumed by the fuel.

2. The quantity of heat produced by a given quantity of fuel is greatest when the combustion is most complete.

3. The combustion is most complete when the filiginous part of the fuel is retained longest in pipes in which it may undergo a second combustion.

4. Of the heat produced none is of use, but what is diffused through the space to be heated, and retained in this space.

5. The temperature in this space will be higher, in proportion as the current of air, which is to renew and keep up the combustion, is less disposed to absorb the heat of this space in passing through it.

* Abridged from the Annales de Chimie, vol. xli. p. 79. C.

Hence

Hence we deduce the following obvious confequences: Corollaries deduced.

1. The fire-place muft be kept feparate from all bodies that conduct heat rapidly.

2. As heat can be produced only by combuftion, and combuftion can be maintained only by a current of air, this current fhould be attracted into pipes, where it preferves the requifite velocity, without going away from the place to be heated; fo that the heat it depofits in it gradually accumulates in the whole of the ifolated ftove, to be afterward given out flowly, according to the laws of its equilibrium

3. When the wood is confumed to fuch a point as to afford no more fmoke, it is of advantage to ftop the outlets of thefe pipes, to keep in the heat, which would be carried into the chimney by the continued current of frefh air, which would neceffarily be of a lower temperature.

4. We fhall obtain a higher temperature, and preferve it longer, under fimilar circumftances, if we conftruct within the ftove, or under the hearth and round the fire-place, pipes in which the air derived from without is warmed before it enters into the apartment to fupport the fire, or to replace what has been confumed. Apertures for heated air.

Thefe pipes are what have been called heat openings, *(bouches de chaleur,)* becaufe inftead of confidering their principal object, it is commonly fuppofed, that they are made to give a more rapid paffage to the heat produced. This is not totally without foundation, fince the temperature of the air iffuing from them is increafed by the heat it abforbs from the ftove; and on this account fome might be difpofed to neglect them, as contrary to the moft effential object, that of retaining the heat in it; but it is to be obferved, that we can fhut thefe outlets when we pleafe; and that we may even cut off all communication with the external air by means of a fimple flider; fo that every advantage may be derived from them without any inconvenience. It muft be added, that they are neceffary in very clofe apartments, unlefs we would expofe ourfelves to currents of cold air. Thefe reafons have induced me to employ the heat openings in the Swedifh ftove, to which they had not been applied. Neceffary in very clofe apartments.

The Swedifh ftoves are conftructed ftrictly according to the trueft principles, and the pipes in which the fmoke circulates are difpofed in the beft manner for effecting its complete combuftion. Swedifh ftoves conftructed on the beft principles.

So ufeful, that they are become general in Sweden; buftion. Their utility has been found fo great, that they have become general in Sweden, where the winters are very fevere, and where they have diminifhed the confumption of wood one third, fo that there is no country where the inclemency of the weather is guarded againft at lefs expence. and are employed in different works. They have likewife been employed advantageoufly, with the neceffary variations of form, in dye-houfes, breweries, &c.

Conftruction; Their conftruction is by no means expenfive; they fave iron-work, and require only bricks or tiles. Thefe are recommended to be placed edgewife, and chofen as thin as poffible for the inner walls. The circulating pipes are to be placed fo, that rain falling down the chimney can never get into them. and method of ufing them. The method of ufing them is fo eafy, that in the largeft public buildings one perfon is fufficient to light all the fires. All the wood that can be contained in the fire-place, which is very fmall, is to be put in at once; it is to be fawn into pieces of equal lengths; and as foon as it is burned, the flider that ftops the communication of the circulating pipes with the chimney is to be thruft in. By thefe means all the heat, which the fuel is capable of producing, remains in the pipes, and iffues out flowly, and only to diffufe itfelf in the apartment; while a fingle piece of wood, that had not burned at the fame time with the reft, would oblige the flide to be left open, and the current of air neceffary for its combuftion would carry off into the chimney the greater part of the heat produced.

Defcription of the Swedifh ftove conftructed by Guyton. The following is a defcription of the ftove conftructed under my directions.

Its height, Fig. 1, Plate III. reprefents a front view of the ftove: its height is 164 centimetres (about 61 inches French), exclufive of the vafe, which is a feparate ornament, merely placed on the top.

breadth, Its breadth is 85 centimetres (about 31½ inches.)

depth. Its depth 58 centimetres (about 21½ inches.)

Height may vary. The height may vary according to the fize of the apartment, and be extended without inconvenience to two metres (about 6 feet, 2 inches.) It may likewife be reduced, as I have done for ftoves in a laboratory, which were to fupport a fand bath as high as the hand.

Proportions of the circulatory pipes. The other two dimenfions are determined by thofe of the bricks employed to form the interior circulatory pipes, which fhould be in certain proportions, that the fmoke may pafs through

through them freely, without fo much air entering with it as would condenfe it, or fink the temperature below the degree neceffary for combuftion.

V V are the external parts of the two heat openings. Heat openings.

m m Apertures of the ftove, by which the air, that is to iffue through the heat openings, enters. Thefe are clofed when the air is drawn from without through a pipe paffing under the floor; which is much more advantageous for renewing the refpirable air of the apartment, and prevents the danger of currents of cold air attracted by the fire; and which is neceffary, as I have obferved, whenever the volume of air in the apartment is not fufficient, to fupply both the confumption of the fire, and the circulation in the heat pipes.

Fig. 2. is a plan of the foundation of the hearth at the height of the line A B, fig. 1. *l l* are empty fpaces, to receive the air, and convey it into the compartments, where it is to be heated before it iffues by the heat openings, whether the air be obtained from without, or fimply by the apertures *m m*, fig. 1.

Fig. 3. pl. IV. plan at the height of the line C D, fig. 1; that above the door of the fire-place, *n n n n* are the double plates of caft iron, forming the compartments in which the air is to receive the effect of the heat of the fire,

o o The empty fpace between thefe plates.

Fig. 4. Front fection at the line I K, fig. 3. The arrows indicate the direction of the fmoke in the circulatory pipes of the front part.*

In this the plates of iron *n n* are feen in their perpendicular fituation, with the tongues which form their compartments on each fide of the fire-place. One of thefe plates is reprefented in front fig. 7.

T is an opening left at the bottom of the fourth circulatory pipe, to reftore the draught of air in the fire-place, if necef-

* Among the number of Swedifh ftoves defcribed and delineated in the collection publifhed by baron Cronftedt there are feveral, the circulating pipes of which pafs under the hearth. This gives them a little more extent no doubt, but as foon as the hearth is covered with afhes, the air paffing beneath can receive but a very flight impreffion of heat; it obliges the fire-place to be raifed higher; and it renders the conftruction more complex and expenfive. For thefe reafons I have adopted the moft fimple plan.

 fary,

fary, by burning there a few flips of paper, or other light combuftible, I fay *if neceffary*, becaufe I have found by experience, that this precaution may be neglected, as foon as the ftove has been heated fo as to have loft all its internal dampnefs.

The door of this fort of blower, or air-vent, ought to fhut very clofe. For this purpofe it is fufficient, to cut a piece of brick of the proper fize, to make a hole in it to receive a handle, and to faften upon it a piece of plate iron projecting a little all round it.

Fig. 5. Plan at the height of the line E F, fig. 1.

Fig. 6. Tranfverfe fection at the line G H of fig. 3, which fhews the height of the fire-place, and the firft direction of the flame.

V points out the arrangement of the heat pipes.

The dotted lines give the profile of the party walls, which form the four grand circulating pipes.

R the pipe which conveys the fmoke from the circulatory pipes into the chimney, and in which is the regifter that cuts off the communication. It is a common ftove tunnel of plate iron; but it would be better to ufe a fubftance more flowly conducting heat, as an earthen tube made on purpofe, for that part in which the flider or ftop plate acts.

The elbow made by this pipe to reach the chimney renders it unneceffary to repeat, that it is a point of the firft importance for the body of the ftove to be completely feparate from the wall. That which I have defcribed is 25 centimetres (about 9 inches) diftant from the neareft point of the nich in which it is placed.

S is an elongation of the perpendicular pipe that enters into the chimney. It is intended to receive the water that might condenfe in the upper part, to prevent it from getting into the ftove. The cap at the end of this elongation allows the pipe to be cleaned without taking it down.

The dotted lines forming the fquare fpace Q mark a place where a nich may be made, or a fort of little ftove, as is done in fome of the Swedifh ftoves, and would advantageoufly fupply the place of the brick-work, with which it muft otherwife be filled up.

All thefe figures being drawn on the fame fcale, there will be no difficulty in preferving the proportions of the parts.

The conftruction of this ftove is neither difficult nor expenfive. For the outfide nothing is wanted but Dutch tiles, fuch as are ufed for common ftoves, that is to fay thin in the middle, and having a border all round, which ferves to give them more ftability. They are fixed in like manner by a band of metal. The hind part may confift entirely of bricks. The vafe placed on the flab of marble or ftone, which covers the ftove is a mere ornament.

Materials of which the ftove is conftructed.

If it be thought proper to have no heat openings, all the interior ftructure may be made of bricks of proper fizes, laid with loamy earth moiftened, and fet on edge for the circulatory pipes, without any iron except a caft plate over the fireplace, and a door and frame in the ufual manner.

The heat openings may be omitted.

The expence of the heat openings however confifts only in four caft iron plates with tongues and grooves to form the compartments reprefented at fig. 7. All the reft is done with plate iron, bent round and rivetted, which, when once enclofed in the mafonry, will not admit the efcape of the air.

The expence however not great.

How are they made.

Caft iron plates with grooves are well known, fince Franklin's ftoves have been adopted. If it were found difficult to procure them, their place might be fupplied in two ways. Firft by portions of pipes of caft iron, which might be placed vertically fide by fide, ferving as the infide walls of the fireplace, and communicating with each other by little channels at top and bottom formed in the mafonry. Secondly, by common plain caft plates, foft enough to admit of being bored, fo as to rivet on bent flips of plate iron, which would perfectly anfwer the purpofe of the tongues and grooves. As thefe would never be expofed to the action of the flame, there is no reafon to fear their cafting. The latter of thefe two methods is obvioufly the moft advantageous, as it occupies lefs room, and yet affords more furface to receive the action of the heat, and communicate it to the circulating air.

Subftitutes for the caft-iron plates.

In concluding my defcription of this ftove I ought not to omit faying, that nearly two years experience has convinced me of the good effects of its proportions.

Its utility confirmed by two years experience.

It is placed in a room fronting the north, the floor of which meafures 47 metres fquare (about 12 toifes $\frac{1}{2}$) and which is 42.5 decimetres (13 feet) high.

Account of its effects.

Every day a log of wood 28 or 30 centimetres (10 or 11 inches) round, fawn into three pieces, or an equal quantity of fmaller wood, is burned in it at once. The flider of the door

of the fire-place is ſhut, and the key R, fig. 6 is turned, as ſoon as the wood is reduced to charcoal. Ten hours after the air throughout the room is at a temperature above the mean; and the centigrade thermometer, placed 36 centimetres (above 13 inches) from the ſtove, riſes rapidly to 16 or 17 degrees.

To ſhew ſtill more plainly to what degree the economy of fuel and preſervation of heat may be carried by this conſtruction, I ſhall relate another experiment, which I have repeated on ſeveral occaſions, and which has always afforded me very nearly the ſame reſults.

The thermometer in the room, in which there was no fire the day before, being between 9 and 10 degrees, a log ſawn in three as uſual was put into the fire-place about eleven in the morning; and at three in the afternoon a ſimilar quantity of fuel was put in.

At four o'clock the thermometer, placed at the diſtance above-mentioned, was at 42 degrees.

At five, at	37 degrees.
At ſeven,	34.
At nine,	31.
At midnight,	26.

You could not bear to touch with the hand the iron rim of the heat openings. The bulb of the thermometer being placed oppoſite one of theſe openings, at the diſtance of 8 centimetres (about 3 inches) roſe in four minutes to 35°.

The next morning at 9 o'clock the thermometer, which had been again placed at the diſtance of 35 centimetres, was at 22°.

Finally at noon, that is to ſay twenty-one hours after the laſt wood was put in, and eighteen hours after the key had been turned, all the wood being reduced to charcoal, the thermometer ſtood between 18° and 19°. It was then placed two centimetres only from one of the heat openings, and in leſs than ſix minutes it roſe to 26°.

Theſe effects are ſo different from what we commonly obtain by the conſumption of three or four times as much fuel, that I may expect more than one reader to ſuppoſe them exaggerated; but I hope a ſufficient number will be found diſpoſed to make a trial of theſe ſtoves, that their teſtimony and example may at length triumph over our habits, and produce a general conviction, that, without ſuffering any privation ourſelves, we may preſerve for our offspring what uſeleſs waſte is daily robbing them of in an article of the firſt neceſſity.

VII. A Con-

VII.

*A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies *; with ſome Experiments and Obſervations on ſolar Light, when imbibed by* CANTON's *Phoſphorus. By* NATHANIEL HULME, *M. D. F. R. S. and A. S.*

SECTION XI.

The Effects of various aerial Fluids on ſpontaneous Light.

INTRODUCTION.

THE apparatus employed for experiments with any kind of air, unleſs otherwiſe expreſſed, conſiſted of the following parts: 1. A tea-ſaucer, holding about three ounces of water. 2. A wide-mouthed phial, which would contain about ten ounces of liquid. 3. A ſmall wooden ſtand, compoſed of a ſlender pillar or pin, nearly four inches high, fixed into a round baſe, a little more than an inch in diameter, and half an inch thick. This ſtand was faſtened by ſtrong thread to the middle of a piece of flat lead, ſuch as lines Chineſe tea-cheſts, having holes in it to admit the thread; the lead was about three inches ſquare, and doubled, to give it weight and ſtability. The top of the pillar was made pointed; and a round piece of cork, about an inch in diameter, and half an inch thick, was fixed upon it, by means of a ſuperficial hole bored in its under part with a gimlet. Apparatus made uſe of,

When the whole apparatus was put in uſe, the phial was filled with cold pump water, in a pneumatic tub, then inverted, and the ſpecies of air to be employed was let up into it, to the quantity of about eight ounces. The ſubject for experiment being applied to, or faſtened upon, the top of the cork, the ſtand was placed on the tea-ſaucer, and then introduced, under water, into the phial containing the air. The whole apparatus, being now ſupported by the tea-ſaucer, and its uſe.

* Phil. Tranſ. 1801. p. 483. The former part is in the Phil. Tranſ. for 1800, page 161, and alſo in the Philoſ. Journal, Quarto, IV. 421, 451.

with

with water in it, was depofited in the laboratory for experiments on light. By this contrivance, the experiments were made in about eight ounces of air, by meafure, confined above two ounces of water.

§ 1.

The Effects of common or atmofpherical Air on fpontaneous Light.

EXPERIMENTS.

The exclufion of atmofpheric air prevents the appearance of fpontaneous light.

Experiment 1. Two frefh herrings were hung up together in the laboratory, fo as to touch each other at their flat fides; and it was obferved that the parts in contact remained dark, while thofe expofed to the open air became very luminous.

Fish.

Exp. 2. Another frefh herring was laid upon a piece of thick brown paper, and placed in the laboratory. On examination, the next evening, the upper part, which was expofed to the air, was very lucid; but the underfide, lying upon the paper, remained quite dark.

Exp. 3. A luminous herring was divided tranfverfely quite through its middle flefhy part; but the infide was perfectly dark. On the following night, that which before was dark had become luminous.

Exp. 4. At 9 P. M. a piece of frefh herring, of about three drams in weight, was introduced above water, into about eight ounces of atmofpherical air. On the fecond night it was become luminous; on the third and fourth, it continued fhining; and on the fifth the light was extinguifhed. This experiment was frequently repeated, with both the flefh of herring and of mackerel, and nearly with the fame refult.

Exp. 5. The cork of the apparatus was well fmeared with the luminous matter of a mackerel, and then introduced above water. It continued to fhine finely all that evening; and the light was not quite extinct on the fucceeding night.

Exp. 6. Another cork was illuminated with herring-light, at half an hour paft fix P. M. and introduced above water. It remained very bright at eleven; and retained a glimmering light the next evening. The two laft experiments were often repeated, and, in general, with fimilar effects. It may not, however,

however, be improper to obferve, that the illumination of the cork did not always continue fo long as twenty-four hours; for it muft, of courfe, vary according to the quantity of luminous matter applied, and its degree of brilliancy.

Exp. 7. A large piece of rotten wood was received from the country, which fhone only in one place. The luminous portion was fawed off for ufe, and the dark part left in the laboratory. On going into the laboratory, the fecond night after this operation, I was furprifed to fee the dark piece, which had been left there, very lucid in feveral places where fmall fplinters had been broken off in fawing; many fhining fragments alfo lay fcattered on the floor. Rotten wood.

Exp. 8. A quantity of rotten wood, moderately fhining, was blown upon for fome time with a pair of bellows; but I could not perceive that this had any effect on the light, fo as to render it more vivid.

Exp. 9. A fmall piece of fhining wood was tied upon one of the corks of the apparatus, and introduced above water, where it continued lucid until the fifth night. In another experiment, the light was extinguifhed on the fourth night; and and in a third much fooner.

Exp. 10. A living glow-worm, in a fhining ftate, was fubmitted to the action of a pair of bellows; but the continuance of the blaft did not apparently increafe its glowing quality. Glow-worm. Thefe are not excited by bellows.

Exp. 11. A very luminous dead glow-worm was fixed upon a cork of the apparatus, by means of a fmall pin, and then put into the phial, above water. It continued to fhine as vividly as it did when in the open air, forming a pure white light, of a circular fhape.

OBSERVATIONS.

Obfervation. 1. Thefe experiments prove, that objects which abound with fpontaneous light in a latent ftate, fuch as the herring, mackerel, and the like, do not emit it when deprived of life, except from fuch parts as have been fome time in contact with the air.

Obf. 2. They likewife fhow, that the blaft of a pair of bellows does not increafe this fpecies of light, as it does that which proceeds from combuftion.

§ 2.

The Effects of oxigen Gas or vital Air on spontaneous Light.*

EXPERIMENTS.

Fish, shining wood and Glow-worms in common air and in oxigen gas exhibited no difference of illumination.

Experiment 1. A piece of fresh herring, of about three drams weight, was introduced above water, into eight ounces of oxigen gas. On the second night it was observed to be faintly luminous; on the third, the quantity of light was increased; on the fourth, it continued nearly in the same state; and on the fifth the light was diminished.

Exp. 2. A piece of very fresh mackerel, of the same magnitude, was also put above water. On the subsequent evening it was pretty lucid, and continued the same on the night following.

Exp. 3. At 9 P. M. a cork, finely illuminated with mackerel-light, was introduced above water: it continued very lucid at eleven. On the next evening it was dark.

Exp. 4. Another cork, rendered luminous with the same kind of light, was put above water at 10 P. M. The next morning, at six o'clock, only a glimmer of light was perceived and at 10 P. M. it was extinct.

Exp. 5. At 9 P. M. a fragment of shining wood was introduced above water; it was observed to be still very luminous at eleven; but the light was not quite so vivid, nor so extended in breadth, as when the wood was put in. On the succeeding night, at eight o'clock, it remained faintly lucid.

Exp. 6. A little after 8 P. M. another fragment of wood, shining very brightly, was introduced above water, into the same air that was used in the last experiment: it continued very luminous at eleven; but the light was diminished in quantity. On the next evening it was found to be extinguished.

Exp. 7. The same air was employed again at 8 P. M. with a pretty large and thick fragment of wood, uncommonly lucid: its light continued vivid and broad at half an hour past eleven. The following night, at eight o'clock, the light was still somewhat extensive and bright.

Exp. 8. In three other experiments with shining wood, in fresh oxigen gas, the light was totally extinguished in the space of twenty-four hours.

* The oxigen gas made use of was obtained from manganese, by means of heat.

Experiments were made, at the ſame time, and in the ſame manner, with atmoſpherical air and ſhining wood; but it was not very evident that the wood ſhone more vividly in the latter air than it did in the oxigen gas.

Exp. 9. A living glow-worm was put into a two-ounce phial, with a glaſs ſtopple, containing pure oxigen gas, and kept therein for ſome time. It was then taken out, and expoſed to the open air; but no difference, either in the brilliancy or the quantity of its light, could be diſcovered.

Exp. 10. A luminous dead glow-worm was then incloſed in about five ounces of the gas; but no increaſe of its ſhining quality could be perceived.

Exp. 11. At ſix o'clock P. M. a ſhining dead glow-worm was introduced above water into oxigen gas: it continued very lucid therein at 7 P. M. ſhewing a pure white light. It was then taken out, and put above water into atmoſpherical air, where it ſhone, to all appearance, as ſplendidly as it did when it was in the oxigen gas.

OBSERVATION.

It appears, from theſe experiments, that oxigen gas does not act upon this kind of light, ſo as to render it much more vivid than it is in atmoſpherical air; which is quite contrary to what ſome authors have alledged.

§ 3

The Effects of azotic Gas on ſpontaneous Light.

1. *Azotic Gas, obtained from lean muſcular Fleſh and diluted nitric Acid, in a very low Heat, as recommended by M. de* FOURCROY.

EXPERIMENTS.

Experiment 1. A piece of freſh mackerel, weighing about three drams, was introduced above water, into about eight ounces of this azotic gas; and it was retained therein five days, without emitting any light.

Azote gas does not admit of the production of ſpontaneous light but maintains it in ſome inſtances for a time if previouſly produced.

Exp. 2. About the ſame quantity of freſh herring was then put above water, into the ſame gas uſed for the laſt experiment, and remained in it for the ſpace of three days, in a dark ſtate. This experiment was repeated, and with a ſimilar reſult.

Exp. 3. At 45 minutes paſt 7 P. M. a cork, finely illuminated with mackerel-light, was put above water into the gas, and it was found pretty luminous at eleven. On the next evening, at eight o'clock, it ſtill exhibited a faint degree of light.

A ſimilar experiment was made, at the ſame time, in atmoſpherical air. At 11 P. M. the cork was but moderately luminous; and on the next evening it was dark.

Exp. 4. At 40 minutes paſt 7 P. M. another cork, rendered very luminous with herring-light, was introduced above water. This cork, at 11 P. M. was not found ſo lucid as that in the third experiment. On the next evening, a glimmer of light was ſtill perceptible.

Exp. 5. A fragment of very ſhining wood was introduced above water, into this gas; and it was rendered dark in about 15 minutes.

Exp. 6. The experiment was repeated; and the light was again extinguiſhed in about 15 minutes. In another experiment it was extinguiſhed in about 25 minutes.

II. *Atmoſpheric Air rendered azotic, by burning Spirit of Wine in it, when confined above Water*

Atmoſpheric azote.

Exp. 7. A portion of freſh herring, of about three drams, was put above water, into this azotic gas, at 5 P. M. On the ſecond evening, a ſpark of light was obſervable; on the third, the quantity of light was increaſed; on the fourth, it was again diminiſhed.

Exp. 8. At 3 P. M. the uſual quantity of herring was introduced above water. On the ſecond night, it remained dark; on the third it was moderately luminous; on the fourth, it was leſs ſo; on the fifth, the light was extinct.

Exp. 9. A piece of freſh mackerel was next put above water, at 11 A. M. On the ſecond evening, it was found to be ſlightly luminous; it remained ſo on the third; on the fourth, it was dark.

Exp. 10. Another piece of freſh mackerel was introduced above water, at 3 P. M. On the ſecond night, it was found to be ſlightly luminous; but on the third, it was dark; and no more light was emitted, though it was kept in the gas for the ſpace of four days.

Exp. 11. A cork, made very luminous with herring-light, was put above water, into this gas, at 20 minutes paſt 8 P. M. and it continued very lucid at eleven. The next evening, at ten o'clock, the light was nearly extinguiſhed.

A ſimilar experiment was made, at the ſame time, in common atmoſpherical air, and with the ſame reſult.

Exp.

Exp. 12. Another cork was introduced above water, with herring-light, at 40 minutes paſt 7 P. M. and it remained pretty luminous at eleven. On the following night, it was nearly extinct.

III. *The laſt mentioned azotic Gas, after being waſhed with Lime Water.*

Exp. 13. A piece of herring, of about three drams weight, was put above water, into this azotic gas, at 5 P. M. On the ſecond night, it was dark; on the third, very lucid; and on the fourth, the ſame.

Exp. 14. The experiment was repeated, on a piece of herring, at 3 P. M. On the ſecond evening, it was dark; on the third, pretty luminous; on the fourth, it was leſs ſo; and on the fifth, only a faint light remained.

Exp. 15. A portion of freſh mackerel was then put above water, at 11 A. M. On the ſecond night, it was obſerved to be moderately ſhining; on the third, the light was extinct.

Exp. 16. Another piece of freſh mackerel was introduced above water, at 3 P. M. On the ſecond evening, it was ſlightly luminous; on the third, it was dark, and continued ſo during the four ſucceeding nights.

Exp. 17. A cork, finely illuminated with herring-light, was next introduced above water, into this gas, at 20 minutes paſt 8 P. M. The light was much diminiſhed at 45 minutes paſt 8; at 11 the cork had become almoſt dark. On the following night, a glimmer was ſtill apparent.

Exp. 18. Another cork, made very luminous with herring-light, was put above water, at 40 minutes paſt 7 P. M. and it continued pretty lucid at eleven. On the next evening, the light was merely viſible.

A ſimilar experiment was made, at the ſame time, in atmoſpheric air, and with nearly the ſame effect.

OBSERVATION.

It is a remarkable circumſtance, that azotic gas, which is incapable of ſupporting light from combuſtion, ſhould be ſo favourable to the ſpontaneous light which is emitted from fiſhes, as to preſerve its exiſtence and brilliancy for ſome time, *when applied upon a cork;* yet that it ſhould prevent the *fleſh* of the herring and the mackerel from becoming luminous, and alſo extinguiſh the light proceeding from rotten wood.

The

§ 4

The Effects of hidrogen Gas or inflammable Air on spontaneous Light.*

EXPERIMENTS.

Hidrogen gas does not admit the production of light.

Experiment 1. At 9 P. M. a piece of fresh herring, weighing about three drams, was introduced above water, into hidrogen gas. It was retained therein three days and three nights, without emitting any light. It was then taken out, and exposed to the action of atmospherical air. On the following night it was found to be luminous; but was dark again on the next night.

Exp. 2. Another piece of fresh herring was put above water, at 6 P. M. This was also kept in the gas the same length of time, without producing any light. It was then exposed to the open air, and inspected two successive nights, but it remained dark.

Exp. 3. The same experiment was then made with a piece of mackerel, which was taken out on the fourth night, without producing any shining appearance. The next evening, it emitted a very faint light, which did not continue twenty-four hours.

but extinguishes it. (Fish)

Exp. 4. A cork, brilliantly illuminated with mackerel-light, was introduced above water; and the light was extinguished in about the space of an hour.

Exp. 5. At 39 minutes past 9 P. M. another luminous cork was put above water; it lost some of its light pretty soon, but was not extinct at twelve.

Exp. 6. A cork, with herring-light, was introduced above water, at 23 minutes past 6 P. M. The light gradually diminished, and was only faintly visible at eleven.

Exp. 7. A fragment of very shining wood was put above water, at 9 P. M. and was dark at eleven.

Common air again revives it.

Exp. 8. Another fragment was put above water, at 40 minutes past 8 P. M. at 50 the light was much diminished, and at 8 minutes past 9 the shining ceased. The wood was then taken out, and exposed to the open air, when the light revived in a very beautiful manner.

* This gas was obtained from zinc and diluted sulphuric acid.

Exp.

Exp. 9. A piece of uncommonly fhining wood was introduced above water, at 58 minutes paft 8 P. M. it remained for a fhort time very luminous, but at 25 minutes paft 9 the light was greatly diminifhed; at 20 paft 10 it was nearly extinguifhed; and at 29 paft 10 was quite dark. It was then expofed to atmofpheric air, and the light revived very brightly.

Repetition of extinction and refufcitation. (Wood)

Exp. 10. The fame experiment was repeated, at 35 minutes paft 8 P. M. the fhining property was much diminifhed at 9; and at 10 it was very faint. The next evening, it continued merely vifible. The wood was now taken out, and the light foon revived very ftrongly. The following night, it was ftill moderately lucid; but on the next evening nearly extinct.

Exp. 11. Finding, by the above experiments, that the light of fhining wood was extinguifhed by this fpecies of gas, and reftored by atmofpheric air, the following three trials were made, to difcover, in fome degree, how long its light might be kept in a latent ftate, and then be revived. At 9 P. M. feveral fragments of fhining wood, tied up in a piece of gauze, were introduced above water, into the hidrogen gas, and the light was gradually extinguifhed during that evening. They were kept there in that dark ftate 48 hours, were then taken out, and expofed to the open air, when, after a little time, the light re-appeared.

Extraction for 48 hours.

Exp. 12. On the 2d of October, another fragment of exceedingly fhining wood, two inches and a half long, and pretty thick, was put above water in the evening, and its light was gradually extinguifhed. On the fecond night, it was taken out perfectly dark, but its light recovered by degrees, and became brilliant. It was introduced again, that evening, into the fame gas, and its light difappeared. On the third night, it was again expofed to the open air, and the light revived as before. It was then reinftated and extinguifhed, and continued in a dark ftate, from the third to the fifth night, when, being again taken out, it foon fhone in a pretty vivid manner. It was again introduced and extinguifhed as ufual; and no obfervation was made of it, from fome accidental circumftance or other, until the 10th of November in the evening, when it was taken out, and expofed to the open air for a length of time, but the light did not revive.

Three repetitions of extinction and renovation.

Exp.

No renovation after a week

Exp. 13. A third fragment, fomewhat larger than the former, and equally luminous, was put above water, at the fame time as the one in the laft experiment, where it was foon deprived of its light. It was retained there, in a dark ftate, from the 2d of October till the 10th of November; it was then taken out, and expofed to the action of atmofpherical air, for feveral days, but there was no return of light.

Three extinctions and renovations of a glow-worm

Exp. 14. About 7 P. M. a fhining dead glow-worm was introduced above water into the gas, and its light was foon extinct. It was then expofed to the open air, where, in a very fhort time, it fhone as brightly as before.

Exp. 15. At half an hour paft 9 P. M. the fame glow-worm was again introduced above water; when its light in a fhort time difappeared. It was taken out for expofure to common air, at 11, and its glowing property was immediately reftored. It was again replaced in the gas, where it foon loft all its light a fecond time, and was kept in that dark ftate for 24 hours; when taken out, it continued dark for a little time, and then the infect gradually recovered its priftine fplendour.

OBSERVATION.

From thefe experiments we learn, that hidrogen gas, in general, prevents the emiffion of fpontaneous light, and alfo extinguifhes it when emitted; but, at the fame time, it does not hinder its quick revival, when the fubject of the experiment is again expofed to the action of atmofpherical air; although the light may have been a confiderable time in an extinguifhed ftate.

(To be concluded in our next.)

Caution

VIII.

Caution againſt the great Danger of keeping Phoſphorus in Bottles without particular Caution. By DESCROZILLES *the Elder.**

I have narrowly eſcaped falling a victim to an accident occaſioned by the effect of the froſt upon a bottle, which contained a hectogramme of phoſphorus, with a quantity of water ſufficient for covering this highly inflamable ſubſtance. This morning, before day-light, ſome books and a cheſt ſuddenly caught fire in the apartment in which I lay, and which is not occupied during the night. It was ſtill fortunate that the two hours of my accuſtomed ſleep were long ſince elapſed. Some ſeconds later, I ſhould have been ſuffocated by the deleterious vapours of the phoſphoric acid. Quickly gaining the door, I called for aſſiſtance, and we ſucceeded in extinguiſhing the fire before it had made any progreſs. Dangerous accident of fire from phoſphorus.

According to all appearance, the froſt which had taken place ſome days before in this piece, had cauſed the bottle to break; but it was ſurrounded with paper, which prevented its falling to pieces. By this means, in proportion as the water ran off by the effect of the thaw, the bundle of phoſphorus, expoſed to the atmoſpheric air, was ſituated under circumſtances the moſt favourable to ignition. The ſides of the bottle performed the office of a ſmall furnace, in which the cylinders of the combuſtible were propped againſt one another. Soon the gradual combuſtion which produces phoſphorus acid was ſucceeded by the rapid deflagration, the reſult of which is phoſphoric acid. How it happened.

Independently of the effects of froſt and of blows, a bottle frequently breaks without any apparent cauſe, and as it were ſpontaneouſly: It appears therefore to me, that in order to obviate an inconvenience which in ſome caſes may prove very ſerious, the beſt means would be to uſe caſes of copper, ſtrongly ſoldered, and lined internally with paper or bran, for incloſing the bottles filled with phoſphorus and water. Caſes of tinned iron would be deſtroyed much ſooner by oxidation, and their ſoldering would be ſuſceptible of detaching itſelf by the effect of a moderate heat. Other cautions.

Finally, it appears to me that this report ought to be as public as poſſible.

* In a letter to the Editors of the Annales de Chimie, No. 123.

IX. IN

IX.

Obſervations in Anſwer to DR. PRIESTLEY'S *Memoir in Defence of the Doctrine of Phlogiſton* *. *In a Letter from* MR. WILLIAM CRUICKSHANK.

To Mr. NICHOLSON.

SIR, *Woolwich, March* 22, 1802.

IN your Journal publiſhed the *1ſt of this month*, I find a letter from Dr. Prieſtley, in which he ſtill defends the old doctrine of phlogiſton, but with very little ſucceſs; for having additional difficulties to ſtruggle with, he has been under the neceſſity of adopting new, and ſometimes contradictory opinions, in his explanations and defence; For example, it is now his opinion that all metallic calces contain water and little or nothing elſe; and that charcoal, uniting with water, forms both fixed and inflammable air; for as this ſubſtance contains the elements of both kinds, nothing but water is wanted to enable them to take the form of air. It is almoſt impoſſible to argue againſt ſuch ſtrange ſuppoſitious and looſe reaſonings as theſe. However, let us ſuppoſe water to be the only ſubſtance contained in oxides; if ſo, heat alone ought to revive at leaſt ſome of them, and in this caſe nothing but water ſhould be ſeparated. Now that heat alone revives ſeveral, particularly thoſe of mercury and the perfect metals, is a fact ſufficiently aſcertained; but, inſtead of water, we obtain the pureſt oxigen gas, the quantity of which added to the revived metal, amounts, as nearly as poſſible, to the weight of the original oxide; even from the ore of mangaueſe, ſo difficult to reduce, a prodigious quantity of the pureſt oxigen gas may be procured by a moderate heat; and when the oxide has been previouſly reduced to powder and well dried, by a heat nearly red, no water whatever can be perceived: in this caſe, too, the quantity of the oxigen gas diſengaged, will be found to correſpond very nearly with the loſs of weight in the oxide.

Obſervations of Dr. Prieſtly, that metallic calces contain little addition except water.

But when reduced by mere heat they afford pure oxigen, very nearly equal to their loſs of weight, and no water.

How is it poſſible, then, that theſe phenomena can be explained on the ſuppoſition, "That all the calces contain little

If calces contain water only, then charcoal muſt produce the ſame

* Philoſ. Journal I. 181,

"or

"or nothing elfe but water?" when not a particle of water is to be feen during the revival, or partial revival, of the metals from their oxides in clofe veffels. Let us, however, confider this fuppofition in another point of view; which is, that if water only were contained in thefe oxides, then the gas obtained from a mixture of them and charcoal, fhould be the very fame as that procured from moiftened charcoal; but the contrary of this I have clearly proved to be the cafe (fee Phil. Journal, vol. v. quarto, p. 6.); and this was one of the principal faults which led to the difcovery of the gafeous oxide.

thing by heat with calx as with water;

contrary to fact.

I fhall here juft enumerate a few of the properties by which thefe gafes may be readily diftinguifhed, being deduced from a number of experiments, often repeated with uniform refults. Firft, then, the fpecific gravity of the gafeous oxide of carbon is no lefs than double that of the gas obtained from moiftened charcoal, being as 30 to 14.5. Secondly, The proportion of oxigen neceffary to faturate the gafeous oxide, is to that required by the hydrocarbonate as 15 to 44.8, or 1 to 3, nearly eftimating by quantity. And, thirdly, which is the moft diftinguifhing property of the whole, the fame quantity of oxigen, fuppofe 14 parts, which, when combined with the gafeous oxide of carbon, produces from 36 to 44 parts of carbonic acid (according to the purity of the oxide), will, when combined to faturation with the hydrocarbonate, produce only 12 parts of the fame acid, accompanied, however, with much water, proceeding in part from the hidrogen in the charcoal, of which all common charcoal, it would appear, contain a certain proportion (fee P. Jour. No. 55. p. 210 and 211, and alfo the Table of Analyfis, &c. p. 8. No. 59.) Surely gafes having properties fo effentially different, can never be confidered as the fame.

Enumeration of differences; fpecific gravity of gafeous oxide of carbon double that of hydrocarbonate;

faturating oxigen very different;

and products of carbonic acid with like portions of oxigen.

Dr. Prieftley remarks, that there is a confiderable difference in the qualities of heavy inflammable air, depending not only on the fubftance employed, but alfo on the ftages or periods of the procefs itfelf*. Now I have found that thefe variations never take place in any remarkable degree but when charcoal is employed in fome form, and that even then the differences chiefly depend upon the imperfect ftate of the charcoal; for if good charcoal be expofed to a red heat in clofe veffels during 10 or 15 minutes, and then mixed whilft hot with the fubftance

Differences in heavy inflammable air noticed by Dr. Prieftley,

are found only when charcoal is prefent,

and more or lefs impure;

* P. Journ. No. 3. (Oct.) p. 183.

to

to be employed, likewiſe hot and completely dried, the variations in the qualities of the gaſes will be much leſs, the principal difference depending upon the proportions of the carbonic acid and the inflammable gaſes; but the gaſes from pure charcoal, either alone or moiſtened, never have any of the diſtinguiſhing characters of the gaſeous oxide, being always much lighter, yielding bulk for bulk when ſaturated with oxigen, not more than one-third of the carbonic acid gas afforded by the other. What, however, diſtinguiſhes them ſtill more completely, is the large proportion of water generated by the combuſtion of the hydrocarbonate in oxigen gas; for the gaſeous oxide when pure, or burned under the ſame circumſtances, never produces the leaſt ſenſible quantity of this fluid.

the hydrocarbonates are much lighter than the gaſeous oxide, and they afford water by combuſtion with oxigen.

Erroneous quotation and remarks.

In mentioning the circumſtances of the production of the gaſeous oxide of carbon from the metallic calces and charcoal, &c. Dr. Prieſtley has ſomehow mis-ſtated both the meaning and words in what he calls a quotation from the firſt paper; this paſſage in his letter is as follows (ſee p. 182.): "After "repeating my experiment, which he found to be juſt, Mr. "Cruickſhank did the ſame with the calces of the other me- "tals, as zinc, copper, &c." and then concludes (p. 4.) "*that* "*in* ALL *theſe caſes the air muſt come from the partial decompo-* "*ſition of the carbonic acid by the calx, when raiſed to a high* "*temperature*:" Then the Dr. goes on and adds: "But the "inference that I think is more naturally drawn from them is, "that all theſe calces contain much water, and little or no- "thing elſe." Now the paſſage from which this quotation appears to have been taken (for there is nothing in p. 4. exactly ſimilar to it), does not follow the account of the experiments with the calces of the metals, &c. but is an inference drawn from the firſt experiments made with the carbonates and the iron ſcales. I ſhall inſert this paſſage, and leave the philoſophical reader to judge for himſelf of the accuracy of the ſtatement and the juſtneſs of the concluſion. "Conceiving "that in theſe experiments * the gaſeous oxide muſt proceed "from the partial decompoſition of the carbonic acid by the "iron when raiſed to a high temperature, I thought I ſhould "ſucceed better by employing iron filings in place of the grey

* Alluding to theſe made with the carbonate of barytes and iron ſcales, or imperfect grey oxide.

" oxide, as these would have a greater affinity for oxigen." From the above statement the misrepresentation both of the explanation and words must be manifest, and the insertion of the word *all* is curious enough. (Essay on Gaseous Oxide, &c. Philos. Journal, vol. v. quarto, p. 4.)

Filings and chalk heated, supposed by Dr. Priestley to afford a gas consisting of phlogiston and water.

In the experiments made with the mixtures of carbonate of lime dried as much as possible and metallic filings, the Dr. conceives that the gas must have been produced by the water which, still remaining in the carbonate, had united with the phlogiston of the metal, and passed over in the form of inflammable gas, mixed with the fixed air separated from the chalk.

If so, the gas ought to be the same as with filings and water;

but this is the lightest of elastic fluids, and *forms water* by combustion;

whereas the gaseous oxide is the heaviest inflammable gas, and produces carbonic gas by combustion, *with no water.*

If the inflammable gases were produced in these cases by the water separating from the earth, and passing over the red-hot metal in the form of steam, then they should be the very same as that obtained by passing the vapour of water through a red-hot iron tube; but it is well known, the gases thus produced are so far from being the same, that they are extremely different in all their properties; for the hidrogen, or light inflammable air, produced by the decomposition of the water in passing through the tube, is the lightest of all aeriform fluids, and, when combined with oxigen, there is not the least appearance of carbonic acid, and nothing formed but water. On the contrary, the gaseous oxide procured by heat from a mixture of the driest earthy carbonates with metallic filings, is the heaviest of all known inflammable gases, and when united with oxigen, produces nothing but carbonic acid, there not being the least appearance of water. It is impossible, therefore, that these heavy inflammable gases should be produced by water alone, in any state, acting upon pure metals; for the gases obtained from the decomposition of this fluid, whether from the solution of metals in dilute acids, or from their action on its vapour when raised to a red heat, are always of the same nature, being pure hidrogenous gas.

Recapitulation of the experiment wherein carbonic acid gas passed over ignited iron gave oxigen to the metal, and was itself converted into carbonic oxide.

Dr. Priestley observes, that before I admitted that the iron or its calx, raised to a high temperature, could decompose the carbonic acid in this experiment, I should have tried whether it would do it in any other. This remark clearly proves that he had not seen the second essay on the gaseous oxide, &c. in your Journal for the month of August, in which a process is described, where this acid is decomposed, even in its gaseous form, by passing it repeatedly through a red-hot iron tube filled

in the middle with iron wire. Bladders filled with this gas were attached, by ftopcocks, to the extremities of the tube. After the middle of the tube, placed in a furnace, had been made red hot, the gas was repeatedly paffed, and very flowly, from one bladder to the other. During this paffage it was expofed to a very extenfive furface of red-hot iron; and in one of the laft experiments made in this manner, after forcing the gas through the tube backwards and forwards 30 times, four parts in five of the carbonic acid gas was converted into gafeous oxide: the gas upon the whole was a little diminifhed. The iron wires, after the operation, were covered with the fame fhining cruft as if the fteam of water had paffed over them; and in fact they were oxidated or calcined to a certain extent, in confequence of feizing from the carbonic acid a proportion of its oxigen fufficient to convert it into the intermediate ftate of an oxide *.

Dr. P. could not decompofe carbonic acid by the burning glafs with iron, becaufe the contact of ignited furface was infufficient.

The method by which Dr. Prieftley attempted to decompofe the carbonic acid gas, by heating pieces of iron with a lens placed in it, could not poffibly have fucceeded, at leaft to any fenfible degree; for the heated air when left at liberty to afcend, would not remain in contact with it, nor even near it, for a moment: thus every portion of air, being unconfined, would expand prodigioufly, fo that very little of it could come or remain in contact with the heated metal. In the experiment above related, the gas was computed to pafs feveral times over a furface of red-hot iron of fome extent; yet, notwithftanding this, a confiderable time was required to produce any remarkable decompofition in the acid; a circumftance which proves that, in the method employed by Dr. Prieftley, no fenfible decompofition could have been produced.

Simpler method of decompofing carbonic acid gas.

There is likewife, in my fecond Effay, another method ftill fimpler for decompofing the carbonic acid in its gafeous ftate †. All the oxidible metals are, when raifed to a high temperature, capable of decompofing it more or lefs; but zinc is by far the moft powerful, owing no doubt to its greater affinity to oxigen.

Though mere heat does not expel oxigen from finery cinder; yet heat and the attraction of charcoal may;

Dr. Prieftley afks, if, in the experiment with the finery cinder and charcoal, the oxigen to form the carbonic acid fhould come from the calx, how is it to be expelled, as heat alone will not do

* For the particulars of this experiment, and the mode of conducting it, fee this Journal, quarto f. No. 55. p. 209.

† No. 55. p. 209.

do it, &c. &c.? But here he has forgotten that the affinities of bodies are remarkably varied by change of temperature, and that, in all reductions of metallic oxides, the carbon of the charcoal, when raifed to a high temperature, unites with the oxigen of the calx in confequence of increafed affinity, and forms with it carbonic acid gas and gafeous oxide. That this is the cafe is remarkably proved by diftilling the red oxides of mercury and lead with charcoal; for thefe oxides we know contain a large proportion of oxigen, which may be feparated by heat alone, but when heated with charcoal, nothing but carbonic acid and gafeous oxide come over, becaufe, in this cafe, the whole of the oxigen being in its nafcent ftate, combines, even at a low temperature, with the charcoal, and paffes over in the form of carbonic acid gas mixed with the gafeous oxide. There are fome circumftances accompanying the diftillation of charcoal with thefe metallic oxides, which are eafily reduced, and at low temperatures, not altogether uninftructive, and, in my opinion, unanfwerable by the fupporters of phlogifton. In thefe proceffes the proportion of gafeous oxide to carbonic acid gas is but fmall, being in general about one-third or one-fifth of the whole, and for the moft part is obtained juft before or at the time the mixture becomes red: It is always accompanied with a torrent of carbonic acid gas. At the inftant the metal is revived, the gas either ceafes entirely, or comes over very flowly; but on increafing the heat, it again makes its appearance, and is now fo far from containing gafeous oxide, that it is peculiarly light, not mixed with any fenfible quantity of carbonic acid gas, and yields, when faturated with oxigen, but a very fmall proportion of this acid gas.

as it does in all reductions.

Red oxides of mercury and lead afford carbonic acid and oxide.

Circumftances of thefe experiments.

Deductions.

The following facts may be drawn from thefe experiments: Firft, It would appear that a much greater degree of heat is neceffary for the proper production of the gafeous oxide than for the carbonic acid. Secondly, That oxigen, in its nafcent ftate, may unite with carbon at lefs than a red heat, and form carbonic acid; as is clearly proved by the procefs with the red oxide of mercury and charcoal. The gafeous oxide appears likewife to be produced at a very low heat.

Conclufion.

I have now taken a view of all the arguments which Dr. Prieftley has brought forward in defence of his former opinions, but fhall at prefent make no further obfervation on this fubject, leaving the argument and fact to be decided by your philofophical and chemical readers.—I am, SIR, &c.

X. AT

X.

On the new Planet CERES.

To Mr. NICHOLSON.

SIR, *Parſon's Green, April 3*, 1802.

The ſubject reſumed.

AT the concluſion of the memoir concerning the new planet Ceres, which you did me the honour to publiſh in your laſt number of the Philoſophical Journal, a want of leiſure, and the length of the communication, were alledged as reaſons for my not concluding, at that time, the whole of the obſervations which I had to offer on the ſubject: I beg leave, therefore, now to reſume the examination and detail of thoſe particulars which remain yet to be treated of.

Diſcovery of a method of deducing the greateſt equation from the eccentricity.

About four years ago, when I was inventing a mechanical contrivance, by which the equation of the center and true diſtance of a planet, or any number of planets, might be exhibited in an orrery, I diſcovered that the *natural ſine of half the greateſt equation of any planet, is equal, or very nearly equal, to the decimal figures which repreſent the value of a vulgar fraction, compoſed of the eccentricity and mean diſtance of that planet:* For inſtance, if we take the mean diſtance of the earth from the ſun at 100000, and the eccentricity, according to Lalande, at 1681,395, the fraction $\frac{1681,395}{100000}$, converted into a decimal expreſſion of the ſame value, is 01681395; and, omitting the decimal point and three laſt figures, we ſhall have 01681 for the natural ſine of 0° 57′ 47,6″, which are differs only about *half a ſecond* from one half of the greateſt equation, as given in the tables of the third edition of Lalande's Aſtronomy.

The proceſs, in the form of an analogy, will be thus: As the mean diſtance : is to unity :: ſo is the eccentricity : to the natural ſine of $\frac{1}{2}$ the greateſt equation.

Tabulated numbers to ſhew its correſpondence in all the planets.

This analogy will apply to all the other planets, as may be ſeen in the ſubjoined table, which I have calculated from the mean diſtances and eccentricities given in Lalande's Aſtronomy, and copied by Mr. Vince, except in the inſtance of Ceres, the data of which planet are taken from the elements of Gauſs.

Planets,

Planets.	Vulgar Fractions.	Decimals, or Nat. Sines.	Correfpondent Arcs.	Half the greateft Equation, 1750.
			° ′ ″	° ′ ″
Mercury	$\frac{7955,4}{38710}$	,20551	11 51 34,9	11 50 0
Venus	$\frac{498}{72333,14}$	,00688	0 23 38,3	0 23 40
Earth	$\frac{1681,395}{100000}$	,01681	0 57 47,6	0 57 48,2
Mars	$\frac{14183,7}{152369,27}$	,09308	5 20 26,9	5 20 20
Ceres	$\frac{8250}{276750}$	,02981	1 42 28,96	
Jupiter	$\frac{250217}{520379,2}$	,04807	2 45 18,6	2 45 19,15
Saturn	$\frac{53640,42}{954072,4}$	,05622	3 13 22,7	3 13 21
Georgian	$\frac{90804}{1918352}$	,04733	2 42 45,5	2 43 38

In this table the greateft difference between the arcs contained in the fame line of the two laft columns, is that in the line of Mercury; but it may be worthy of remark, that half the greateft equation of this planet, according to Dr. Halley's tables, is 11° 51′ 18″; and alfo that Lalande himfelf made a new determination of the elements of Mercury's orbit, as related in the "Memoires de l'Inftitute Nationale" of Paris for the "fourth year of the Republic;" in which the grand equation is given 23° 40′ 45″.

Remark concerning mercury.

Here, then, it appears, that the greateft equation of the new planet Ceres, which correfponds to the eccentricity affigned by Gaufs, and copied into the differeut Journals, is about 3° 25′, inftead of 9° 27′ 41″; fo that, as I hinted before, either the eccentricity is almoft two thirds *too little*, or the greateft equation almoft two thirds *too much*. I mean not at prefent to enter into a geometrical demonftration of the analogy which I have ufed in procuring the above tabulated refults; but leave it to exercife the ingenuity of your mathematical readers, fome of whom will probably be induced to favour you and the public with a demonftration, as a feparate article. I will, however, juft prove to the reader the accuracy of the inference I have made with refpect to Ceres, by means of the *elliptic hypothefis* of Ward, which is generally allowed to be a convenient approximation to be ufed for finding the equation of a planet, inftead of either the direct or tentative methods, which are more accurate, but much more intricate.

Hence the greateft equation of Ceres, as affigned, does not correfpond with its eccentricity.

Proof of the accuracy of this inference, by computation on the elliptic hypothefis of Ward.

By the elliptic hypothefis, the analogy for converting mean into equated anomaly is fimply this: *As the aphelion diftance : is to the perihelion diftance :: fo is the tangent of half the mean anomaly : to the tangent of half the equated anomaly*; and the difference between thefe two anomalies conftitutes the equation itfelf. Now, it is well known to all who are converfant in the theory of planetary motion, that in the projection of any elliptic orbit, a *circle*, defcribed from the focus in which the fun is fuppofed to be, with a radius that is a mean proportional between the major and minor femi-axes, will *cut the ellipfe* in *two points*, which fhall be the points of *mean diftance*; or, which is the fame thing, the points where the equation becomes *ftationary*, and confequently where it is a *maximum*. It is alfo equally well known to practical aftronomers, and calculators of an ephemeris, that the equation varies very flowly for many degrees both before and after the points of mean anomaly correfponding to the greateft equation; and likewife that thefe points fall a little beyond the firft quadrant from the aphelion, or three degrees of mean anomaly, by a quantity which depends upon the eccentricity of the orbit. In the orbit of Mercury the point of mean anomaly, when the equation is greateft, is nearly at 105° from the aphelion; in that of Venus it is between 90° and 91°; in that of the Earth about 91°: in that of Mars about 97°; in that of Jupiter and Georgian between 93° and 94°; and in that of Saturn about 94°. Hence it may be inferred, that if the greateft equation of Ceres be 3° 25′, the faid point of mean anomaly will be about 92°: but that if the equation be 9° 27′ 41″, it will be about 96°; namely, fomewhat fhort of that of Mars, the greateft equation of which is 10° 40′ 40″.

Let us try now what the greateft equation will be upon both fuppofitions fucceffively, according to the *fimple elliptic hypothefis*.

		Log.
As the aphelion diftance	(27673+825) 28498	4,45481
Is to the perihelion dift.	(27673—825) 26848	4,42891
So is the tangent of 46°	$(\frac{92°}{2})$ $\frac{1}{2}$ mean anom.	10,01516
		14,44407
		4,45481
To the tangent of $\frac{1}{2}$ eq. anom. 44° 17′ nearly		9,98926

Then 92°—88° 34′=3° 26′ is the greateft equation.

Again,

Again, ſuppoſing the point of mean diſtance to be at 96°, we have in that caſe,

As the aphelion diſtance, 28498 - - - - - -	4,45481
Is to the perihelion diſt. 26848 - - - - - -	4,42891
So is the tangent of 48° - - - - - - - -	10,04556
	14,47447
	4,45481
To the tangent of 46° 17′ nearly - - - - -	10,01966

Then 96°—82° 34′=3° 26′ is the greateſt equation, as before.

Hence it is indubitably proved, that the equation, as given by Gauſs, is much too great for the eccentricity; and it appears alſo, according to what has been already aſſerted, that the equation at 92° and 96° of mean anomaly is *nearly the ſame*; that is to ſay, the difference will only be in the *ſeconds*. But the greateſt equation of a planet is uſually determined from a ſeries of obſervations antecedently to the calculation of the eccentricity: therefore the error which has been detected may be in the eccentricity, and not in the equation; in which caſe, by reverſing the analogy already uſed, we ſhall have this calculation, viz. As unity : mean diſtance 27673 :: natural ſine of 4° 43′ 50½″ or 08247,4 : 2282,2 for the requiſite eccentricity. But it will be moſt eaſy to determine in which of the two elements the error has been committed, when the whole period has been accurately aſcertained.

The equation given by Gauſs is too great for the eccentricity; or the eccentricity is faulty: but this will be beſt known from the period, when determined.

When it was mentioned in the former paper on this ſubject, that oppoſitions and conjunctions were of importance to be obſerved, the reaſon was omitted to be explained; which is, that when a ſuperior planet is in oppoſition, or an inferior one in conjunction, the obſerved geocentric longitudes are alſo heliocentric longitudes, without calculation or reference to diſtance and eccentricity; becauſe in ſuch relative ſituations there is no parallax of the orb: and it is well known to aſtronomers, that when an oppoſition happens at the place of mean diſtance of a ſuperior planet, *half the difference* between the heliocentric place, by calculation of mean motion, and of the place as obſerved at that time, is equal to the *greateſt equation*. The 13th of March ult. was the day on which the aſtronomers on the continent predicted that an oppoſition of Ceres would

Oſervations of conjunctions and oppoſitions, and their uſe.

 occur;

occur; but it muſt have happened on the 23d, as I calculate from Von Zach's little ephemeris continued forwards; viz. when the geocentric plane was about 182°. The aſtronomer, who has an obſervatory, and has noted the *exact time*, will do well to make the obſervation public.

Application to the planet Ceres.

The mean time which elapſes between two ſucceſſive oppoſitions or conjunctions of a planet, as ſeen from the earth, is called a ſynodic revolution, and is determined by dividing 360° by the difference of the mean daily motions of the earth and other planets. Thus: taking the mean daily motion of Ceres at 770,7376″, according to Gauſs, and of the earth at 58′ 8,33″, according to Lalande, we have $\frac{360°}{2777,5924}$ =,466,6 days nearly for the whole ſynodic period, on a ſuppoſition that the motions are both equable throughout their orbits; but their reſpective diſtances from their aphelia at the time of oppoſition muſt be made the argument of a correction, either additive or ſubtractive, as the caſe may be, to determine what a ſynodic period would be if both motions were equable. Now, if we reverſe this proceſs, we can juſt as eaſily gain the difference of the daily motions between that of the earth and other planets, and conſequently the whole period of the latter, from having only the earth's daily motion, and *obſerved ſynodic period*; for 360°, divided by this period in days, gives the difference wanted at once, which, ſubtracted from the daily motion of the earth, gives that of the other, if it be a ſuperior planet; but if an inferior one, that difference muſt be added; and the more nearly the two daily motions approximate to each other, the longer will be the reſpective ſynodic revolution. In the inſtance before us, if we ſuppoſe the whole corrected ſynodic revolution of Ceres to be 466,6 days from obſervation, we ſhall have $\frac{360°}{466,6}$ = 2777,5924″ for the *difference* to be ſubtracted from 3548,33″ the earth's mean daily motion, which will leave 770,7376″ for the mean daily motion of Ceres, as before; by which if we divide 360°, we ſhall have the whole tropical period = $1681^d\ 12^h\ 8^m\ 49^s$. But it remains to be obſerved what a whole ſynodic period of Ceres *may prove in reality*.

Determination of the real period, &c.

Suppoſing the epoch, or mean heliocentric longitude of Ceres to have been 2 S. 17° 36′ 34″ on January 1, 1801, the day of its diſcovery, as is ſtated by Gauſs, and the place of the aphelion 10 S. 26° 27′ 38″, the mean anomaly muſt, on

 this

this ſuppoſition, have been at that time 3 S. 21° 8′ 56″, ſo that it had paſſed the place of mean motion about either 19° or 15°, accordingly as we make the greateſt equation 3° 25′ or 9° 27′ 41″: therefore the daily motion was nearer a mean motion than it has been ever ſince; and it will be yet ſome months before it arrives at its place of mean motion in the oppoſite half of its orbit; which place is either 2° or 6° ſhort of the *ninth* ſign of anomaly, accordingly as we take the eccentricity. Let us ſuppoſe now the whole period to be upwards of 1681 days, as has been, perhaps prematurely, determined; one fourth of this time had elapſed on the 24th of February laſt; on which ſuppoſition, the *mean anomaly* muſt then have been advanced juſt *three ſigns* from the original ſituation; namely, it muſt have been upwards of 6 S. 21°, at which rate the planet had paſſed the perihelion by a ſpace of time anſwering to 21° of mean motion, which is about 98 days: therefore the 18th of November, 1801, muſt have been the day on which it was at the perihelion, or place of greateſt velocity; but at that time the planet *was loſt*, and we are not in poſſeſſion of any obſervation of it nearer that time than the 7th of December following, when Baron Von Zach re-diſcovered it.

The continuance of any planet in the firſt quadrant from aphelion is longer than in the ſecond quadrant, by a ſpace of time which correſponds to the whole equation, taken at three ſigns of mean anomaly; in which ſituation, it has been already obſerved, that the equated or apparent motion is alſo, as nearly as may be, a mean motion; if therefore the equation at three ſigns be divided by the mean daily rate of motion, we ſhall have a ſpace of time, which, added to one fourth of the whole period, and ſubtracted from another fourth, will give nearly the reſpective times of continuance in the firſt and ſecond quadrants of anomaly: Hence ariſes this rule for finding the two ſemicircles, reſpectively biſected by the perihelion and aphelion points, viz. divide four times the equation at three ſigns of anomaly, (which may be the greateſt equation where the eccentricity is ſmall), by the mean daily motion, and the quotient will be the number of days that the planet continues longer in the ſemicircle from nine to three ſigns of anomaly than from three to nine. For inſtance, if we take the equation

tion of Ceres at 3° 25′, we ſhall have $\frac{3^\circ\,25' \times 4}{77^\circ\,7376''} = 63{,}83$ days for the time of continuance in the ſemicircle embracing the aphelion, longer than in the ſemicircle which is biſected by the perihelion: but if we take the equation at three ſigns = 9° 25′, ſomewhat leſs than the greateſt equation, in this caſe, by reaſon of the increaſed eccentricity, we ſhall have the exceſs of continuance $\frac{9^\circ\,25' \times 4}{77^\circ\,7376} = 175{,}93$ days.

Obſervations from which the poſition of the apſides are deduced, &c. &c.

This ſuggeſtion may be worthy the notice of the practical aſtronomer; for when a variety of obſervations are taken of the new planet in the different quadrants of its orbit, and the correſponding times recorded, it will be no difficult taſk, when equidiſtant geocentric longitudes are converted into heliocentric longitudes, to obſerve what ſemicircle of the ecliptic correſponds to that half of the orbit in which the planet has *continued longeſt*: the middle of that ſemicircle will be the *aphelion*, and the two extremities will be three and nine ſigns of anomaly: Alſo, the *exceſs of duration*, above the time occupied by the other ſemicircle, multiplied by the *mean daily motion*, will be *four times* the equation at three and nine ſigns of anomaly very nearly; and as this equation is very little ſhort of the greateſt equation, the eccentricity may likewiſe be found by either of the methods already deſcribed: Thus the form and elementary points of the orbit may be gained by a ſeries of obſervations converted into heliocentric places, even by the projection propoſed in the laſt memoir on this ſubject, and theſe determinations may be corrected by a compariſon of them with the reſults deduced from the properties of an ellipſe, which are here purpoſely omitted, leſt a more minute and ſcientific deſcription of intricate calculations ſhould rather puzzle than inform the generality of readers *.

Inclination of the orbit, and place of nodes.

It remains yet that ſome obſervations be made relative to the *poſition* of the orbit of a planet. There are many methods of aſcertaining the nodes of a planet's orbit, from calculation grounded upon obſervations; but the ſimpleſt, when it is practicable, is to convert the geocentric into the heliocentric place at the time when there is no latitude by obſervation, for

* See Lalande's and Mr. Vince's Aſtronomy; and alſo Profeſſor Robiſon on the Geo. Sidus, in the Edin. Tranſ. vol. I. 1788.

the

the heliocentric place will be the place of the node, afcending or defcending, as the cafe may be, which will appear by a fubfequent obfervation; but when the place of a planet, when croffing the ecliptic, cannot be obferved, the *middle point* between two *equal north* and *fouth* latitudes, gained by obfervation, will give the node.

The heliocentric latitude, when a planet is juft 90° from each node, is the meafure of the inclination of its orbit, and is eafily obtained from the obferved geocentric latitude, taken in that fituation, by the analogy already defcribed; or, otherwife, the greateft heliocentric latitude may be acquired from an obfervation of a geocentric latitude and longitudinal diftance from the node, thus: When the earth is in the line of the nodes, the analogy will be, *as the fine of the difference of the longitudes of the fun and planet feen from the earth* : *radius* :: *tangent of the geocentric latitude* : *tangent of the inclination.*

The two days on which the earth will be in the line of the nodes of Ceres will be June 12. and December 13. this year. But it is beyond the propofed intention of this popular memoir to enter into all the minutiæ of calculation, were the requifite data before me; but only to point out the *methods* of applying obfervations for determining the fize, form, and pofition of a planet's orbit: it may not, however, be unworthy of notice, before I conclude, to remark, that the aftronomers on the continent, who availed themfelves of the earlieft obfervations *only* for determining an approximate fet of elements of Ceres, were enabled to do this from noticing that this planet became *ftationary* between the 10th and 11th of January, 1801, when its elongation was known by obfervations; for it has been fhewn by writers on aftronomy, that, upon a fuppofition of circular orbits, *the tangent of the elongation is equal to the femidiameter of the orbit, divided by the fquare root of that femidiameter* + 1.

Days when the earth will be in the line of the nodes of Ceres.

The early determinations refpecting the orbit of Ceres were made from its ftationary pofition.

Your's, &c.

W. P.

APPENDIX.

APPENDIX.

April 10, 1802.

Since the preceding paper on the planet Ceres was written, Mr. Ed. Troughton has put into my hands the duplicate of another letter, fent to him by the Baron de Zach, and addreffed to Sir Jofeph Banks, Bart. which I underftand has been read at a meeting of the Royal Society, and which I fhall here tranfcribe *, on account of fome remarks which I have to make upon it. (Copy,)

Seeberg Obfervatory near Gotha, Feb. 20, 1802.

" Dear Sir,

etter from Ba-n von Zach.

" I had the honour to fend to you my obfervations of the new planet Ceres Ferdinandea made in January, here I take the liberty to fend the continuation of them made in February.

able of obfer-tions.

1802.	Mean Time in Seeberg.			Ap. Right Afcen. obferved.			App. Dec. obferved.		
Feb. 3	15h	40′	55″ S.	188°	42′	13,05″	12°	40′	5″ N.
4	15	36	41,4	188	42	36,30			
5	15	32	45,1	188	42	30,15	12	50	25
9	15	16	43,7	188	38	3,90	13	14	18
19	14	34	46,7	187	58	27,90	14	20	3

Dr. Gaufs has corrected his elliptical elements of the orbit upon my obfervations; here is what he has found fince my laft letter to you.

lements.

Epoch for the beginning of the year to the meridian of Seeberg - - - - - 77° 27′ 36,5″

Aphelion } both fidereal, - - 325 57 15,0
Node } - - - - 80 58 40,0

Greateft equation of the center - - 9 20 8,0

Inclination of the orbit - - - 10 37 56,6

Logarithm of ½ axis major 0,4424742

Eccentricity of the orbit 0,0814064

Mean diurnal heliocentric and tropical motion 769″,7924

With thefe elements of the orbit all the obfervations made by Mr. Piazzi in Palermo, from Jan. 1, till Feb. 11, 1801, agree

* The letter was fent open to Mr. Troughton for the exprefs purpofe of copying and communicating the fame. W. N.

agree perfectly well, and within a few feconds; and my obfervations are reprefented by them thus:

Seeberg obferv.	R. A. calculated.	Differ.	Declin. calcul.	Differ.
1801, Dec. 7	178° 33′ 29,2″	— 1,4″		
1802, Jan. 11	186 45 47,6	— 2,3		
16	187 27 38,8	—14,4		
22	188 6 18,2	— 7,6	° ′ ″	
25	188 20 37,2	— 2,0	11 56 58,4	+35,4″
26	188 24 37,0	—12,5		
28	188 31 25,7	—12,1	12 9 55,6	+14,3
29	188 34 14,1	— 4,0		
30	188 36 38,4	— 5,5	12 19 19,3	+19,1
31	188 38 38,3	— 7,1	12 24 15,3	
Feb. 3	188 42 7,8	— 5,2	12 39 53,6	—11,4

As thefe elements agree hitherto fo well with the heavens, the following ephemeris calculated upon them for the next month, will probablydo the fame, fo I annex it here to point out to the Englifh obfervers the place where they have to look for the Ceres.

Pofition of the Ceres for Midnight Mean Time in Seeberg Obfervatory. Ephemeris.

1802.	R. A. in degrees.	Decl. N.	R. A. in Time.
March 1	186° 41′	15° 30′	12ʰ 26′ 45″
4	186 11	15 50	12 24 45
7	185 39	16 10	12 22 36
10	185 5	16 29	12 20 18
13	184 28	16 47	12 17 53
16	183 51	17 4	12 15 24
19	183 13	17 19	12 12 50
22	182 34	17 33	12 10 15
25	181 55	17 44	12 7 40
28	181 17	17 54	12 5 7
31	189 39	18 1	12 2 37
April 3	180 3	18 6	12 0 12
6	178 29	18 10	11 57 54

This planet will come in oppofition to the fun, March 17 in the afternoon. At the fame time this heavenly body will be in its greateft proximity to the earth = 1,6025, and therefore

fore the moſt favourable time to look for its ſatellites, if there are any, to meaſure its diameter; and to examine its nebuloſity. About this time, the planet will alſo be in its greateſt geocentrical latitude = 17° 9′, and a little later ſhe will have her greateſt retrograde motion, about 13 min. in right aſcenſion per day. The north declination will increaſe till the beginning of April, and about the 9th of the ſame month the motion in declination will commence to the ſouth. It appeared to me that the Ceres has ſome change of light; I imputed it at firſt to our hazy atmoſphere this winter, but Mr. Schroeter of Lilienthal, and Mr. Olbers of Bremen, ſent me word that they have obſerved the ſame, and they believe that it is the planet which is ſubject to ſuch changes of light. Mr. Herſchel will tell us beſt whether it is ſo. I have ſome hopes to find the planet in ancient catalogues of ſtars. Mr. Meſſier was very near it in the year 1779. The famous comet of that year ran juſt over the northern wing of Virgo, as now, and the new planet was not very far diſtant. If the comet had attained two months ſooner the completion of Virgo, Mr. Meſſier muſt infallibly have obſerved the Ceres then, becauſe he determined all the little ſtars in the vicinity of the comet; the planet would have been in the way of the comet, and ſo of courſe he would have catched the little planet in 1799.

Variable light of Ceres.

If my obſervations are acceptable to you, dear Sir, only a little hint, and I ſhall continue with pleaſure to give you further intelligence.

I am,
with the greateſt eſteem and regard,
very reſpectfully,
moſt honoured SIR,
Your obedient humble Servant,
FRANCIS BARON DE ZACH.
Lieut. Col. and Director of Seeberg Obſervatory near Gotha Saxony."

REMARKS.

Remarks.

1. The diſtance which correſponds to the logarithm of ½ axis major, viz. 0,4424742, is 2,769964, the earth's radius being *unity*.

2. The whole tropical period from the mean daily heliocentric tropical motion, 769,7924″, is 1683ᵈ 13ʰ 41′ 56,5″.

3. The

3. The synodic revolution corresponding to this motion is $466^d\ 10^h\ 22^m$.

4. The time of opposition could not be on the 17th of March as stated in this letter, but about the 23d, as has been mentioned before, because it was on that day that the difference of the right ascensions of the sun and Ceres was 180°, even according to the Baron's own table: the error seems to have arisen from reckoning the point diametrically opposite Ceres to be nearly two degrees *short of* the equinoctial point, instead of the same quantity *over*, when the right ascension of Ceres was about 182°: the other circumstances also dependent on the moment of opposition must therefore be attributed to the 23d instead of the 17th.

Cause of the apparent disagreement of some deductions of the author, and the elements of Mr. Gauss.

5. On receiving these last corrections of Dr. Gauss I was at first surprised to find such a trifling alteration made with the greatest equation and corresponding eccentricity, after the error which I was confident I had detected; but I have now found out the cause of the apparent discrepancy, which some stress has been laid upon; the mean distance and eccentricity, I now perceive are, contrary to the usual mode of expression, given in terms of *different denominations:* the mean *distance* has been given in terms which suppose the radius of the *earth's orbit to be unity*, and the *eccentricity* is given in terms which suppose the radius of the *orbit of Ceres to be unity*, instead of its *proportional* radius 2,769964. Professor Robison on the contrary, in his approximate elements of Georgian expressed the mean distance and eccentricity in terms of the same denomination, which is also done by Lalande, Vince, and other eminent astronomers with respect to the other planets. Let us try now what the greatest equation will be by the elliptic hypothesis, when *unity* is made the radius of the orbit:

As the aphelion distance (1+,08140) 1,08140—4,03383
Is to perihelion distance (1—,08140) 9186—3,96313
So is tang: of 46° ½ mean anom: - 10,04556

4,00869
To tang: ½ eq. anom: 43° 20′ 33,6″ - 4,03383

9,97486

Then 48° 20′ 33,5″ + 2 = 86° 41′ 72″; and 92° — 86° 41′ 7,2″ = 9° 18′ 5,23″ is the greatest equation.

2 Also

Alſo by the tabulated method we have $\frac{8140}{100000}$ = 08140, which is the natural ſine of 4° 40′ 8,27″, or half the greateſt equation 9° 20′ 16,51″, which is not 9″ above the correction of Dr. Gauſs.

6. Hence it appears, that the eccentric point in the projection of the orbit of Ceres ſhould be a little leſs than $\frac{1}{12}$ of the radius from the central point S (Plate XIII. Fig. 1.) which repreſents the ſun.

W. P.

XI.

Deſcription of a very cheap Engine for raiſing Water. In a Letter from Mr. H. SARJEANT *of Whitehaven, to* Mr. TAYLOR, *Secretary to the Society for the Encouragement of Arts.**

SIR,

Introduction.

I AM ſenſible that the little engine, a drawing of which accompanies this letter, can lay no great claim to novelty in its principle; nevertheleſs it is reſpectfully ſubmitted to the conſideration of the ſociety, how far its ſimplicity, and cheapneſs of conſtruction, may render it worthy of their attention, with a view to its being more generally known and uſed in ſimilar caſes.

Height of Irton-Hall 60 feet above the ſtream.

Irton-Hall, the ſeat of E. L. Irton, Eſq. is ſituated on an aſcent of ſixty or ſixty-one feet perpendicular height; at the foot of which, at the diſtance of about 140 yards from the offices, runs a ſmall ſtream of water. The object was to raiſe this to the houſe for domeſtic purpoſes.

To this end a dam was made at a ſhort diſtance above, ſo as to cauſe a fall of about four feet; and the water was brought by a wooden trough, into which was inſerted a piece of two-inch leaden pipe, a part of which is ſeen at A, plate 2.

Deſcription of the engine. A bucket is ſuſpended at one end of a beam and a counter-weight at the other end. The ſtream fills the bucket and raiſes the counter-weight;

The ſtream of this pipe is ſo directed as to run into the bucket B, when the bucket is elevated; but ſo ſoon as it begins to deſcend, the ſtream flows over it, and goes to ſupply the wooden trough or well in which the foot of the forcing pump C ſtands, of three inches bore.

D, is an iron cylinder attached to the pump rod, which paſſes through it. It is filled with lead, and weighs about

* From the Tranſactions of the Society, for 1801, page 255. The ſilver medal was given to the Inventor.

240 lbs.

240 lbs. This is the power which works the pump, and forces the water through 420 feet of inch pipe from the pump up to the houfe.

and this laft in its defcent works a fmall force-pump.

At E is fixed a cord which, when the bucket comes to within four or five inches of its loweft projection, becomes ftretched and opens a valve in the bottom of it, through which the water empties itfelf.

The bucket is made to empty by a valve and ftring.

I beg leave to add, that an engine, in a great degree fimilar to this, was erected fome years ago by the late James Spedding, Efq. for a lead-mine near Kefwick, with the addition of a fmaller bucket which emptied itfelf into the larger, near the beginning of its defcent, without which addition it was found that the beam only acquired a libratory motion, without making a full and effective ftroke.

To anfwer this purpofe in a more fimple way, I conftructed the fmall engine in fuch manner as to finifh its ftroke (fpeaking of the bucket end,) when the beam comes into an horizontal pofition, or a little below it. By this means the lever is virtually lengthened in its defcent in the proportion of the radius to the cofine, of about thirty degrees, or as feven to fix nearly, and confequently its power is increafed in an equal proportion.

Contrivance to complete the ftroke.

It is evident that the opening of the valve might have been effected, perhaps better, by a projecting pin at the bottom; but I chofe to give an exact defcription of the engine as it ftands. It has now been fix months in ufe, and completely anfwers the purpofe intended.

The only artifts employed, except the plumber, were a country blackfmith and carpenter; and the whole coft, exclufive of the pump and pipes, did not amount to £5.

It is very cheap.

I am, Sir,

Your humble fervant,

H. SARJEANT.

Warwick Court, Holborn.

Mr. CHARLES TAYLOR.

In another letter, dated Whitehaven, April 28, 1801, Mr. Sarjeant further obferves that the pump requires about eighteen gallons of water in the bucket to raife the counter-weight, and make a frefh ftroke in the pump; that it makes three ftrokes in a minute, and gives about a half-gallon into the ciftern

With a fall of 4 feet and confumption of 18 gallons, it raifes half a gallon thro' 60 feet. That is to fay 12 parts of

water raise 5 parts. Its rate is about one eighth part of one man's work; and it throws up 24 hogsheads in the day.

tern at each stroke. He adds, "I speak of what it did in the dryest part of last summer; when it supplied a large family, together with work-people, &c. with water for all purposes, in a situation where none was to be had before, except some bad water from a common pump which has been since removed. But the above supply being more than sufficient, the machine is occasionally stopped to prevent wear, which is done by merely casting off the string of the bucket valve."

XII.

Concerning the Identity of Tellurium and Antimony, the galvanic Effects of Magnetism, and other Philosophical Subjects. By a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

History of the intelligence.

ANXIOUS to learn the particulars as well as the truth of the intelligence which I received from Moravia concerning the *Tellurium* and the *decomposition of water* pretended to have been effected at Vienna *by means of the magnetic fluid*,* I applied to a chemical friend residing there; and it appears that this intelligence was not correct. Whence to prevent farther misinformation I conceive it my duty to hasten in publishing the following extract from my correspondent's answer to my letter, and to request you the favour of inserting it in your excellent Journal, if there be room for it.

I have the honour to be,

Sir,

Your humble servant,

N. N.

London, April 21. 1802.

Extract of a Letter from Vienna, dated 30th March, 1802.

"The intelligence communicated to you from Moravia concerning the Tellurium and decomposition of water was a little premature, and is not farther true than as follows:"

Account of the experiments that render it probable that Tellurium is antimony.

"As to the first point Mr. *Tcharsky*, major in the artillery, (well known by his refutation of Tondi's experiments† to-

* See *Philosophical Journal*, new Series, vol. I. pag. 234.

† An account of this is given in Baron *Born*'s systematic catalogue of the collection of fossils of Mlle de Raab at Vienna. Transl.

wards

wards eftablifhing the poffibility of reducing barytes, lime and magnefia to metallic reguli,) had refolved to employ his confiderable ftock of *Tellurite* and *Uranite* for the purpofe of fubjecting thefe two metals to a new and clofer examination. He, therefore, firft prepared, according to Klaproth's manner ‡ a regulus of Tellurium, weighing feveral grains. This regulus perfectly agrees with reguline antimony, *only* with refpect to its *phyfical properties*, for inftance, in the fracture, colour, hardnefs, fpecific gravity; and fo likewife its oxyde perfectly refembles the *antimonium diaphoreticum ablutum* (lixiviated white oxyde of antimony by nitre.) However, it would be premature, without farther experiments, in which Major Tfcharfky is at prefent engaged, directly to conclude that Tellurium is nothing elfe but reguline antimony."

Uranium.

"Major Tfcharfky has alfo prepared many ounces of the oxyde of Uranium, and will in a fhort time reduce it to a regulus of confiderable fize for the purpofe of farther experiments."

Suppofed galvanic effects of the magnet.

"Concerning the fecond point it relates only to an experiment of Profeffor *Jordan*, who, as in the galvanic experiment for decompofing water, connected metallic wires immerfed in water with the oppofite poles of a magnet, and obferved that an oxydation of the wires was effected. But as this experiment would not fucceed in the feveral inftances in which it was repeated, Major Tfcharfky and Captain *Lethenyey* are at prefent occupied in preparing a large magnetic apparatus in order to inftitute a regular feries of experiments on this fubject."

J.

Landriani's method of copying old MSS.

Poftcript. Chevalier *Landriani* affirms that he has difcovered a method of copying old writings in the fame manner, as of recently written papers, duplicates are taken by means of copying-machines.—He has alfo prepared an excellent indelible ink, the compofition of which he will foon publifh.

Indelible ink.

‡ The procefs by which Klaproth eftablifhed the Tellurium as a new metal has been given in *Nicholfon*'s Journal, vol. II. 4to. 1799, page 273. Tranfl.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Notice respecting the Discovery and Situation of chromated Iron in France.

Chromate of iron.

C. Pontier has already found, three years ago, in the lower Alps, some fragments of chromated iron, out of its place; but circumstances and the war prevented him from discovering the true position of this new and curious mineral in the earth. He, however, at last found it in its natural place in a quarry near Gassin, in the road to Cavalaire.

This metal is mixed with a green serpentine rock, which probably owes its colour to the chrome, according to C. Pontier's opinion. It sometimes forms masses of five solid decimeters each.—*Bulletin des Sc.* No 57.

BOOKS OF SCIENCE,

A Treatise on Astronomy, in which the Elements of the Science are deduced in a natural order from the Appearances of the Heavens to an Observer on the Earth; demonstrated on Mathematical Principles; and explained by an Application to the various Phenomena. By OLINTHUS GREGORY, *Teacher of Mathematics, Cambridge. Octavo, 522, very full Pages, with nine Copper Plates, exclusive of the Preface, Table of Contents and Index,* Kearsley, London.

IT must be admitted that we have few works in our language in which the extensive science of astronomy is fully treated; and though we have several excellent popular compendiums, yet a work was wanted in which a familiar deduction of the effects from the phenomena should be combined with the stricter principles of mathematics, and in which a large quantity of useful and interesting matter should be given in the compass of an octavo volume. These are the leading advantages of the present work, besides that of its recent appearance, which naturally enabled the author to enrich his treatise with the interesting discoveries of later times.

His order consists in an investigation of the figure and dimensions of the earth; the use and meaning of the imaginary circles and points; apparent motions; causes of seasons, &c. the fixed stars; parallax, refraction, equation of time; systems of astronomy; truth of the Copernican system; theory of apparent motions; law of the planetary revolutions; real motions, rotations; phenomena of secondaries; eclipses, transits, cometary phenomena; aberration of light; elements of computation, tables, &c. &c. L. L.

A

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AND

THE ARTS.

JUNE, 1802.

ARTICLE I.

On the Rev. Mr. Pearfon's Analogy for deducing the greateft Equation from the Eccentricity. In a Letter from Mr. OLINTHUS GREGORY.

Cambridge, May 7, 1802.

To Mr. NICHOLSON.

SIR,

THE brilliant difcoveries which have been lately made by aftronomers in different parts of Europe, have naturally produced a fpirit of inquiry in many perfons who have hitherto paid little regard to fcientific fubjects, and, at the fame time, have excited an earneft and active folicitude in thofe who have made confiderable advances in the caufe of fcience, that thefe inquirers be rightly directed in their purfuit. Among thofe who have thus laudably exerted themfelves in the promotion of ufeful knowledge, your ingenious correfpondent the Rev. Mr. Pearfon, of Lincoln, (now of Parfon's Green) muft certainly be enumerated; and his many able communications to your Journal demand the thanks of its numerous readers. In the laft of that gentleman's valuable papers on the planet *Ceres Ferdinandia*,

Interefting difcoveries in aftronomy.

Mr. Pearfon's method of deducing the greateft equation.

Ferdinandia, he mentions a ſimple method of deducing the greateſt equation of a planet's centre from the excentricity, which he diſcovered a few years ago; and he invites your mathematical correſpondents to give a demonſtration of this method, reſpecting the accuracy of which he ſeems to have little doubt. In conſequence of Mr. Pearſon's invitation, I take the liberty of addreſſing you on the ſubject. It will ſave much circumlocution to refer to a diagram: let, therefore, the ellipſis A P G Q in the annexed figure 1. Pl. VII. repreſent the orbit of a planet moving about the ſun S in one of the foci; and let A N Q E be a circle deſcribed upon the major axis of the orbit as a diameter. Then, if P be the place of the planet in its orbit, A N will meaſure the excentric anomaly; alſo, if the arc A D be taken proportional to the time from the aphelion, it will repreſent the mean anomaly; and, letting fall the perpendicular S T from S upon N C continued, it is ſhewn, by *Keill* and many others, that $AN + ST = AD$. It has alſo been ſhewn that, when the equation of the centre is a maximum in any orbit, the diſtance S P of the planet from the ſun is $= \sqrt{SA \times SQ}$, and when this is the caſe, in all orbits of ſmall eccentricity the point P nearly coincides with G, or the place of the planet will be near an extremity of its orbit's minor axis. In this ſituation of S P, it is manifeſt that S T will be almoſt coincident with S C, and nearly equal to it; S D and C N will be nearly parallel; and $DCN + SNC$ nearly equal to $2\,DCN$; that is, the greateſt equation will be nearly equal to $2\,ND$, or $2\,ST$, or $2\,SC$. But, when G S the mean diſtance repreſents the radius of a circle, as the angle G C S is 90°, S C will manifeſtly repreſent the *ſine* of an angle: therefore, as G S, the mean diſtance, to S C, the eccentricity, ſo is 1, or radius, to the ſine of $\frac{1}{2}$ the greateſt equation, which is the ſame as Mr. Pearſon's analogy.

Demonſtration of its ground and accuracy.

It is obvious from this inveſtigation, that the ſimple method pointed out by Mr. P. is merely an approximation. It ſuppoſes that the orbits are nearly circular, and will therefore produce a reſult deviating moſt widely from the truth when the eccentricity is the greateſt. Thus in Mr. P's table (p. 49, No. V. N. S.) the greateſt error is in the orbit of mercury, where the eccentricity is ·20551 in terms of its own mean diſtance. The next greateſt error is in the orbit of Mars, where the eccentricity is ·09308. The next error in order is that

It is merely an approximation.

that of Ceres, where the eccentricity is ·08140, and error $4\frac{1}{2}''$. (Pa. 60. No. V.) In the cafe of the Georgian the error or difference, as ftated in the table, is much too great to correfpond with the eccentricity. But here I conceive there is a miftake in the determination of the eccentricity: for, according to a mean from various aftronomers (*Mechain*, *Hennert*, *De la Place*, *Zach*, and *Robifon*,) the proportional mean diftance of the Georgian is not 1918352, but 1908352. Hence then, we have $\frac{90804}{1908352} = \cdot0475824$ = nat. fine of $2^\circ\ 43'\ 58{\cdot}4''$, differing $\frac{4}{10}$ of a fecond from the determination of *La-lande*. And, in the cafe of Jupiter, where the eccentricity is ·04807, the correfponding difference is $\frac{55}{100}$ of a fecond; which is a little larger than the former, as it ought to be. So that it appears, as well from Mr. P's table as from the inveftigation, that this method does not furnifh a refult fufficiently accurate except when the eccentricity is pretty fmall.

Before I conclude, I would juft beg to remark, that this method, though new to Mr. Pearfon, is in reality well known. An analogy which is, in fact, the converfe of this, though exprefled rather lefs commodioufly, is given with a demonftration, in La Caille's Aftronomy, art. 147. Vince's Aftron. art. 231. and my Aftron. art. 344. And I believe it is alfo given in the excellent work of La Lande; but as I have not that performance at hand, I cannot now refer to the place. Another analogy precifely to the fame effect, and deduced from the fame principles, may be feen in art. 249. Book V. Robertfon's Navigation, and under the article ECCENTRICITY in Dr. Hutton's Math. and Phil. Dictionary. It muft afford great fatisfaction to a gentleman of Mr. Pearfon's candour and liberality, to find that the analogy which he accidentally ftruck out, is the converfe of one which admits of a fimilar demonftration, and has been long ufed by aftronomers as an eafy and excellent approximation.

The converfe analogy to Mr. Pearfon's long known.

I am, Sir,

Your's with much refpect,

OLINTHUS GREGORY.

II.

On the Conſtruction of the Beams of Steam Engines. By Mr. J. C. HORNBLOWER. *From the Author.*

DEAR SIR,

Hiſtorical introduction.

I BEG leave through the means of your Journal, to lay before the public an account of the framed lever mentioned at the cloſe of the article *Carpentry* in the ſupplement to the Encyclopedia Britannica, as it was originally deſigned for an engine to have been erected at Amſterdam in the year 1776, together with two others, poſſeſſing every poſſible advantage of levers conſiſting of ſmall ſcantles.

The framed lever here deſcribed needs no hole bored in it. Other particulars.

I know not by what means the lever above referred to come to be conſtructed with the diſadvantages intimated by the writer of that article, but there is no neceſſity for a hole to be bored, or a bolt to be driven in any part of the framing between the arches, except for the chain ſtays. The wedges *a, b*, Fig. 1, Plate V. thus applied, would be an improvement, for want of which a lever of this ſort in the hands of a negligent engineman had one of its joggles forced off, the ſhoulder of the tennon, which was morticed into the arch, not being a joint by the eighth of an inch, or more; but when it met with the arch it went no further, and continued to work for many years under a great load, and much to its diſadvantage in other reſpects.

Dimenſions of a lever of indifferent workmanſhip.

The length of this lever was 21 feet, the ſcantles were 12 inches by 6; height of the whole when put together 30 inches, and leverage on the gudgeon as 4 to 3. This laſt circumſtance operated much againſt its conſtruction, by giving additional force againſt the joggle at that end, but had it been framed ſix inches higher, I doubt not but it would have ſtood to this day under all its diſadvantages.

Scantles and load.

The ſum of the ſcantles is 18 by 12, area of the ſection 216 inches, column of water in four lifts 4800 lb. with 440 fathoms of rods, (pump rods) which with the appendages on the other end, added to the power neceſſary to overcome the reſiſtance, amount to about ſeven tons.

Another conſtruction Fig. 2, well made.

But a much ſimpler, and in ſome reſpects a more advantageous mode of framing is ſhewn at Fig. 2, and may be conſtructed

ſtructed with or without arches. This, with little variation, is the invention of a Dutch gentleman, and was applied to the load of a 52 inch cylinder (an atmoſpherical engine) ſet up with advantages, which in point of workmanſhip at that time was perhaps not equalled, and therefore may be ſaid to have been fairly tried.

This engine was calculated to raiſe 60,000 gallons per minute, and the ſcantles were 18 by 12, and 12 by 8, and where ſuch timber can be had, it is hardly to be expected to have a lever with greater advantages than this for a ſingle ſtroke, and where a double ſtroke is required, it may be doubled for that purpoſe, retaining all its principles and properties as in Fig. 3. which I ſuppoſe needs no explanation. [Dimenſions and load.] [Double conſtruction.]

Fig. 4. is a lever conſtructed by an eminent engineer in Hungary ſome years ſince, which poſſeſſes a very great degree of ſupport by the king poſt and iron braces, but does not, in my opinion, diſcover ſo much ſcience as the two preceding ones. I forgot to obſerve, that inner arches may be attached to Fig. 1. without materially affecting its principle, if they are well let on the whole framing, and bolted to each other without paſſing through the ſcantles. [Strong framed lever in Hungary.]

I am, SIR,

Your moſt obedient Servant,

J. C. HORNBLOWER.

Eaſt Row, City Road, Tueſday, May 11, 1802.

III.

On the Theory of Chemiſtry. In a Letter from the Rev. J. PRIESTLEY, *L. L. D. F. R. S. &c.*

To Wm. NICHOLSON, Esq.

DEAR SIR,

IN October laſt I ſent you a reply to Mr. Cruickſhank's obſervations on one of my arguments in ſupport of the doctrine of phlogiſton, in which I think I clearly ſhewed that he ſuppoſed fixed air to be formed in circumſtances in which it is impoſſible that it ſhould be formed, and that it is decompoſed by a ſubſtance which has no ſuch power. Having juſt received [Reference to former letter. Vol. I. p. 181.]

ceived a letter from a friend in Paris, in which I find that great account is made of the obſervations of Mr. Cruickſhank, ſo that it is now taken for granted that I muſt accede to the new theory, I beg you would add to my former letter, that Mr. Cruickſhank himſelf abandons the moſt fundamental principle of that theory, which is, that the only ſource of inflammable air of any kind is water; and he makes it not neceſſary for that purpoſe. Mr. Lavoiſier, treating of the inflammable air from charcoal and water, which is ſimilar to that from charcoal and finery cinder, ſays, (*Elements of Chymiſtry, p.* 87 *of the Engliſh tranſlation*) " It cannot poſſibly be diſengaged from the charcoal, and muſt conſequently be produced from the water." According to the new theory, the union of *oxigen*, which is ſuppoſed to come from the finery cinder, with carbon from the charcoal, muſt form *fixed air*, and not any kind that is inflammable. Mr. Cruickſhank therefore muſt abandon the new theory, in order to maintain his peculiar hypotheſis.

Mr. Cruickſhank abandons water as the ſole ſource of inflammable air, which Lavoiſier thought eſſential to his theory.

Inference.

If I do not receive a better defence of this new theory from its able ſupporters in France, I ſhall conclude it to be incapable of defence, and that, as becomes ingenuous men, they will abandon it, as Mr. Cruickſhank has virtually done.

I am, dear Sir,

Your's ſincerely,

Northumberland, Feb. 20, 1802. J. PRIESTLEY.

P. S. I have not any Number of your Journal of a later date than that for April laſt.

IV.

Experiments upon the tanning Principle, and Reflections upon the Art of Tanning. By CIT. MERAT GUILLOT, *Apothecary at Auxerre.*

Prouſt's proceſs for obtaining tanin tedious.

THE tedíouſneſs of the proceſs indicated by Mr. Prouſt for obtaining the tanin, induced me to make ſome experiments, and to endeavour to find a more ſpeedy method of procuring it: the following is the reſult of my inquiries:

1. I

1. I infufed tan, in the ftate of fine powder, for fome hours in water; I filtrated this folution, and treated it with lime water; I obtained a precipitate in confiderable abundance, which I collected upon a filtre, dried, and afterwards treated with alcohol, in order to afcertain whether it were foluble in this menftruum; but the alcohol was not even coloured by it.

Infufion of tan affords a precipitate by lime water.

2. Wifhing to afcertain whether the lime had a greater affinity for the acids than for the tannin with which it was combined, I treated four drachms of the precipitate of which I have fpoken above, with nitric acid diluted with water, with the acid of a very gentle heat; a pretty brifk effervefcence took place with a difengagement of carbonic acid gas; after four hours infufion, I filtrated the liquor, which had affumed a very deep tinge, and there remained upon the filtre a black pulverulent fubftance, brilliant, having an acerb and very flightly bitter tafte; this refiduum weighed a little lefs than two grammes.

Acids difengage a pulverulent matter from the lime;

3. In order to afcertain whether the nitric acid had diffolved any lime, I treated the liquid which I had filtrated with the acidulous oxalate of potafh, and I obtained an abundant precipitate; on which account I conjectured, that fince the nitric acid had diffolved the lime, the fubftance which I had obtained upon the filtre muft be tanin, as the precipitate obtained by the mixture of the lime-water and of the infufion of tan, was produced by the union of the tanning principle and of the lime. In order to afcertain this point, I treated one portion of it with water, and the other with alcohol; I let thefe fubftances infufe in the fand bath for twenty-four hours: the water became ftrongly coloured, and the alcohol more fo; but all the tannin (hitherto I only prefume that it is fuch) was not diffolved; the alcohol diffolved only a little more than half of it, and the water lefs. I treated thefe two liquids, after having filtrated them, with a folution of glue, and I obtained a precipitate fimilar to that which is obtained by mixing infufion of tan with the fame folution, but of a much darker colour, and a little lefs elaftic. When I treated them with the muriate of tin, I obtained a precipitate which became gelatinous; when I treated them with lime-water, the tanin combined with the lime, and reproduced the tanate of lime already formed.

by combining with the lime,

and the pulverulent matter is tanin.

According

According to thefe properties, could I doubt that this was pure tanin? certainly not, fince it prefents the fame refults as that obtained by the procefs of Mr. Prouft.

How obtained pure.

If we wifh to obtain more pure tanin than that which is obtained after the folution of the lime of the tanate of this fubftance by an acid, the infufion in alcohol may be evaporated, and we fhall then have very pure tanin.

The muriatic acid has prefented me with the fame refult as the nitric.

Conjectures that lime water is advantageous in taning by the lime combining with the tan.

The rapidity with which the upper leathers of fhoes (*cuirs d'empeigne*) are tanned, according to the procefs of Citizen Lequin, who, in manufacturing them, contents himfelf with merely fubjecting them to the preparations of wafhing and flefhing by lime water, without fuffering them to fwell, and afterwards tans them, led me to prefume that in this cafe a combination is effected of tanin with the lime contained in the fkin thus treated, befides the combination of the tannin with the gelatine contained in the fkin, which accelerates this fabrication. May it not be probable according to this notion, that the fabrication of leather would be accelerated, if after having fubjected the fkins to the operations of wafhing and flefhing in lime water, they were left to fwell in the fpent ooze or water in which the old bark, which has already ferved for tanning leather, has been infufed. In this cafe, the fmall quantity of tanin diffolved in this water would combine with the lime with which the fkin would be charged in proportion to the working, and would form a tanate of lime. The fwelling would perhaps be effected by this means with a little lefs celerity than by the fulphuric acid, but then it would perhaps be preferable, from the circumftance that the fkin in fwelling would begin to charge itfelf with tanin, whereas by the fulphuric acid, the lime with which the fkin is impregnated, when worked,—this fubftance diffolved in water, the lime, I fay, combines with the fulphuric acid employed to fwell it, which, I prefume, muft give to the leather a brittle quality that it would perhaps not have if the other procefs were ufed. Perhaps alfo, after the fkins have fwelled in the ooze, this completion might be haftened by putting them firft into the folution of tan, as Cit. Lequin does, and afterwards fteeping them alternately in lime water and in infufion of tan, always taking care to leave them but a fhort time in the lime-water, which

which might alter them if they were left in it too long. In this cafe, the lime-water with which the leather would charge itfelf, would determine a more fpeedy precipitation of the tanin, and its union with the lime as well as with the gelatine contained in the leather. I believe alfo that by this means the leather would acquire more weight, a quality in requeft amongft the tanners, and perhaps it would become lefs permeable to water.

Thefe are only conjectures which I advance, as they appear to me to be dictated by the theory of the art of tanning. In this inftance probably, as in many others, the practice will not correfpond with the theory. I recommended the trial to manufacturers, and fhall make it myfelf when a favourable opportunity offers. *Annales de Chimie, No.* 123.

V.

On the Deftruction of the Grub of the Cock-chafer. By EDWARD JONES, *Efq. of Wepre-Hall, in Flintfhire* *.

THE grubs of the cock-chafers (or brown beetles) are white, about an inch in length, and of the thicknefs of a turkey's quill. When difturbed they contract their length, and their bodies dilating, appear like lumps of white fat †, fomewhat oval. Defcription of the grub of the cockchafer.

They inhabit fandy and light loamy foils, lie from about two to fix inches deep, and may be found in fpring, by paring off the fods. Situation.

This place was much infefted by brown beetles; but about twelve years ago, fome labourers removing a bank of earth, expofed a bed of grubs, feveral paces in length. Many of them were fcattered among the fallen foil; and one of the men propofed to ftrip the furface of the bank, which being done, the grubs were feen lying in irriguous channels, as if the parent infects had dropped the eggs moving in various directions.

* Extracted from a letter to the Society for the Encouragement of Arts. Tranf. for 1801.

† Hence the Britifh name " Earth-Lard."

The

They are the favourite food of moles,

The ſame man informed me that they were the favourite food of moles; and he deſired me to obſerve an end of the bank not ſtripped (being covered with mole hills); "for there no beetle grubs would be found." When opened, his remark proved true:—the moles had traced all the labyrinths of the grubs.

which on this account deſerves protection.

I took the hint for the preſervation of my foliage, and have ever ſince protected the race of moles. The brown beetles gradually decreaſed, and are now rarely ſeen here. I have not obſerved more than one or two ſtragglers in the two laſt ſprings.

General habits of moles.

Some notice of the habits of moles may be acceptable to the Society, as it has been ſaid "that they penetrate deep into the earth in dry weather; rarely quit their ſubterraneous dwellings, aud have few enemies;"—and "that they do great miſchief in gardens and corn-grounds."

They are much on the ſurface while the graſs is high.

I have always found that in hay and paſture grounds, as ſoon as the graſs is high enough to cover them, they run upon the ſurface, where they find their food in the numerous caterpillars and inſects which in the early part of the ſummer crawl out of the earth; and they continue above ground till the harveſt. They are frequently cut by the ſcythe; and I have ſeen them at various times come out of deep hay graſs into places recently mown, and, perceiving their expoſure, endeavour to conceal themſelves in the ſhorn graſs.

Do not dig deep;

I have alſo often ſeen moles on very cloſe mown graſs, and bare ſpots in paſture land, plunge, when alarmed, among the roots; following their path (which was diſcernible by the heaving of the ſurface), I have forced them out occaſionally, to try the depth of the covering, which was only a few ſhreds of roots.

except to avoid the plough or ſpade, &c.

There are two circumſtances that may oblige moles ſometimes to penetrate deeply:—diſturbed ſoils in ſummer, ſuch as in gardens; and ploughed light lands, where the moles delve in purſuit of worms; and, in their courſe, they muſt unroot and deſtroy ſome plants; but a vigilant gardener and huſbandman will prevent much damage.

or to eſcape froſt, &c.

The other cauſe of their digging deep is froſt, which they avoid, or it would kill them. I have found them in winter, in peat ſoil, two and three feet below the ſurface; and in the hard froſt of 1794-5 (cutting deep trenches to ſeparate grounds,)

grounds), I found moles feveral mornings, that had worked through and fallen into the trenches, frozen to death.

Their fummer emerfion is proved by the birds of prey: they deftroy great numbers of moles. This year there were taken out of one kite's neft twenty-two moles, and out of another fifteen, fome of which were putrid; befides many frogs and unfledged birds.

Birds of prey deftroy great numbers while above ground.

The rapacity of the kites fhews that they are deftructive enemies to the moles, which, if moles are ferviceable to man, fhould be known, that he may ftay his arm.

Moles are frequently found dead upon the grafs in fummer, with marks of having been bitten, as if to fuck their blood, but with no part of their bodies confumed. This, I fuppofe, is done by weafels; and the following (not very common) occurrence, which happened in the fummer of 1789, tends to prove it:—

A kite was obferved rifing from the ground with fome prey, and inftead of flying to an adjoining wood, he foared almoft perpendicularly. After remaining a fhort time ftationary, he came gradually down, with his wings extended and motionlefs, and dropt very near the place from which he had rifen *.

Incident of a kite deftroyed by a weazel, &c.

Several perfons who were near, and faw the flight and defcent, ran immediately to the fpot, and a weafel darted from the kite, which they found dead; and they difcovered, on examination, that the kite had been bit in the throat, and bled to death. Near it they found a dead mole, yet warm, which was bitten in the neck; and they concluded that the weafel had caufed the death of both.

In feveral parts of the kingdom where I have met with a great number of brown beetles, moles were regularly deftroyed; and in Staffordfhire, being fhown feveral large trees covered by beetles, and totally defoliated, I enquired whether they deftroyed the moles? The anfwer was, that they did, or they fhould be over-run with them.

Brown beetles are abundant where the moles are deftroyed.

The lofs of foliage not being of great confequence to the farmer, he is fatisfied that his turkeys make him amends in other refpects, by eating the brown beetles, of which they are very fond, and which they fwallow voracioufly.

Other remarks.

* A fimilar circumftance was mentioned in the Chefter papers, three or four years ago.

A gen-

A gentleman informed me lately, that rooks alfo eat the beetles.

But thefe means are confined to the winged beetle. It appears to me that the mole is the only certain deftroyer of the grub.

My hay and pafture grounds are, every fpring, thickly ftudded with mole hillocks. They are fcattered in the ufual manner; and when the graffes are up, the moles ceafe to work, and fcarce a hillock appears till after harveft.

VI.

Methods of diminifhing the Irregularities of Time-Pieces, arifing from differences in the Arc of Vibration of the Pendulum. By Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Lynn Regis, May 17, 1802,

Chronometers, &c. rendered irregular by the oil.

AFTER all the improvements which the mechanifm of clocks and watches has received, there are ftill obftacles which ftand in the way of an exact performance. The ufe of oil in chronometers has long been complained of, but ftill remains a neceffary evil; and that variation which obtains in the arc of vibration of the pendulum, is a fource of much error in clocks.

Tranfit clock at Greenwich varies in its arc of vibration.

The tranfit clock at Greenwich fometimes varies in its femi-arc of vibration *twenty* minutes in a year, and a compound pendulum which came under my own infpection fome years ago, varied in its arc nearly as much.

Old remedy for the inequality of the arcs.

Many years have elapfed fince it was difcovered, that the fhort vibrations were performed in lefs time than the long ones, and methods were ufed to remove the inconvenience. HUYGENS propofed a method by which the centre of ofcillation might be made to vibrate in the arc of a cycloid, and demonftrated that a pendulum moving in that curve, would perform all its vibrations, whether long or fhort, in equal times: and others propofed to remove the evil by a peculiar form of the pallats.

These

These contrivances, how ingenious soever they may appear in theory, have not answered any good purpose in practice; but, what may be impracticable to perform by one method, may not be found so by another.

Of little practical utility.

The spring by which the pendulum is suspended seems capable of receiving such improvement, as would render the long and short vibrations isochronal, without any other alteration in the mechanism of the clock. This improvement consists in cutting the spring broader at the top than at the bottom, that it may not bend totally at the point of suspension. The pendulum would, in that case, when in motion, become shorter at the ends of the arc, consequently it would no longer vibrate in the arc of a circle, and on this account there is some reason for supposing that the long vibrations would be performed in less time than they were before.

Proposed remedy by accommodating the flexibility of the spring of suspension.

When this method is put in practice, the spring should be made so broad at the point of suspension at first, that the long vibrations may be performed in less time than the short ones, and then made narrower till all the vibrations be performed in equal times. But I supposed that this method would be attended with much trouble, and that it would be better *to remove the cause than to counteract its effects.*

Method of adjustment.

When I made observations on the pendulum above-mentioned, I found that it was much influenced by the weather. In cold and moist weather it moved through a larger arc, than when the atmosphere was dry and warm.

Influencing cause,

As all metals alter their dimensions, and consequently their elastic force by heat and cold, it follows that that part of the clock called the *crutch*, would act more forcibly on the pendulum in cold weather than in warm; and it seems probable, that this variation would be greater in a weak *crutch* than in a strong one.

supposed to operate by the crutch.

About seven years ago having a regulator made by Mr. James Bullock, of Holborn, and being desirous of putting my theory to the test of experience, I had the *crutch* made exceedingly strong, and firmly connected with the pallats, that the maintaining power of the clock might be communicated to the pendulum uniformly in all states of the atmosphere. The clock case is made very thick of solid mahogany, and upon the *rising board* *, which is two inches thick, stands an an-

Experiment with a regulator.

* The board on which the wheel-work stands.

gular

gular piece of brafs (thus Λ) to fupport the pendulum. This fupport is made faft with fcrews to the *rifing-board* at the bottom, and to the *frame-plate* of the clock at the top. Who invented this fupport I know not, but it appears a very excellent method of giving ftability to the point of fufpenfion of the pendulum.

Refult.

This clock has now been going near feven years. It vibrates 1° 49′ on each fide of the perpendicular, from which I have not feen it vary more than 2′, except once in a very hard froft, and as it feldom varies fo much as 2′ in its femi-arc, this caufe of error in the pendulum feems to be very nearly removed. It has been cleaned only once fince it came into my poffeffion, but this made no alteration, either in its arc of vibration or its rate of going.

I am, with much refpect,

Sir,

Your very humble fervant,

EZEKIEL WALKER.

VII.

On the Theory of Light and Colours *. *By* THOMAS YOUNG, *M. D. F. R. S. Profeffor of Natural Philofophy in the Royal Inftitution.*

Importance of general principles in fcience.

ALTHOUGH the invention of plaufible hypothefes, independent of any connection with experimental obfervations, can be of very little ufe in the promotion of natural knowledge; yet the difcovery of fimple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and univerfal laws, muft ever be allowed to be of confiderable importance towards the improvement of the human intellect.

Object of the prefent differtation.

The object of the prefent differtation is not fo much to propofe any opinions which are abfolutely new, as to refer fome theories, which have been already advanced, to their original inventors, to fupport them by additional evidence, and to apply them to a great number of diverfified facts, which have hitherto been buried in obfcurity. Nor is it abfolutely ne-

* In fupport of the truth of that hypothefis, which afcribes the phenomena to undulatory motions of an extremely elaftic and rare fluid. From the Philof. Tranf. 1802.—N.

ceffary

ceffary in this inftance to produce a fingle new experiment; for of experiments there is already an ample ftore, which are fo much the more unexceptionable, as they muft have been conducted without the leaft partiality for the fyftem by which they will be explained; yet fome facts, hitherto unobferved, will be brought forwards, in order to fhew the perfect agreement of that fyftem with the multifarious phenomena of nature.

The optical obfervations of Newton are yet unrivalled; and, excepting fome cafual inaccuracies, they only rife in our eftimation, as we compare them with later attempts to improve on them. A further confideration of the colours of thin plates, as they are defcribed in the fecond book of Newton's optics, has converted that prepoffeffion which I before entertained for the undulatory fyftem of light, into a very ftrong conviction of its truth and fufficiency; a conviction which has been fince moft ftrikingly confirmed, by an analyfis of the colours of ftriated fubftances. The phenomena of thin plates are indeed fo fingular, that their general complexion is not without great difficulty reconcileable to any theory, however complicated, that has hitherto been applied to them; and fome of the principal circumftances have never been explained by the moft gratuitous affumptions; but it will appear, that the minuteft particulars of thefe phenomena are not only perfectly confiftent with the theory which will now be detailed, but that they are all the neceffary confequences of that theory, without any auxiliary fuppofitions; and this by inferences fo fimple, that they become particular corollaries, which fcarcely require a diftinct enumeration.

Excellence of the optical obfervations of Newton.

Phenomena of thin plates.

A more extenfive examination of Newton's various writings has fhown me, that he was in reality the firft that fuggefted fuch a theory as I fhall endeavour to maintain; that his own opinions varied lefs from this theory than is now almoft univerfally fuppofed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a fimilar form in his own works; and this by no lefs a mathematician than Leonard Euler, whofe fyftem of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huygens, and Malebranche.

Newton firft fuggefted the theory of undulation.

Euler.

Thofe who are attached, as they may be with the greateft juftice, to every doctrine which is ftamped with the Newtonian approbation, will probably be difpofed to beftow on thefe confideration

Reference to the works of Newton.

confiderations fo much the more of their attention, as they appear to coincide more nearly with Newton's own opinions. For this reafon, after having briefly ftated each particular pofition of my theory, I fhall collect, from Newton's various writings, fuch paffages as feem to be the moft favourable to its admiffion; and, although I fhall quote fome papers which may be thought to have been partly retracted at the publication of the optics, yet I fhall borrow nothing from them that can be fuppofed to militate againft his maturer judgment.

HYPOTHESIS I.

A luminiferous Ether pervades the Univerfe, rare and elaftic in a high degree.

Paffages from Newton.

Hypoth. I. Univerfality of the ether.

" The hypothefis certainly has a much greater affinity with " his own," that is, Dr. Hooke's, " hypothefis, than he feems " to be aware of; the vibrations of the ether being as ufeful " and neceffary in this as in his." (Phil. Tranf. Vol. VII. p. 5087. Abr. Vol. I. p. 145. Nov. 1672.)

" To proceed to the hypothefis: firft, it is to be fuppofed " therein, that there is an ethereal medium, much of the fame " conftitution with air, but far rarer, fubtler, and more ftrongly " elaftic. It is not to be fuppofed, that this medium is one " uniform matter, but compounded, partly of the main phleg- " matic body of ether, partly of other various ethereal fpirits, " much after the manner that air is compounded of the phleg- " matic body of air, intermixed with various vapours and ex- " halations: for the electric and magnetic effluvia, and gravi- " tating principle, feem to argue fuch variety." (Birch. Hift. of R. S. Vol. III. p. 249. Dec. 1675.)

Arguments propofed by Newton in fupport of the ether.

" Is not the heat (of the warm room) conveyed through the " vacuum by the vibrations of a much fubtiler medium than " air? And is not this medium the fame with that medium by " which light is refracted and reflected, and by whofe vibra- " tions light communicates heat to bodies, and is put into fits " of eafy reflection, and eafy tranfmiffion? And do not the vi- " brations of this medium in hot bodies, contribute to the in- " tenfenefs and duration of their heat? And do not hot bodies " communicate their heat to contiguous cold ones, by the vi- " brations of this medium propagated from them into the cold " ones?

Mr. Boswell's improvements in the hydraulic Engines

Fig. 3. Pressure Engine.

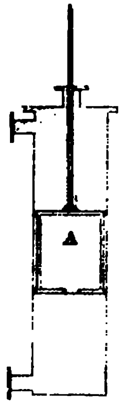


Fig. 2. Mr. Goodwyns.

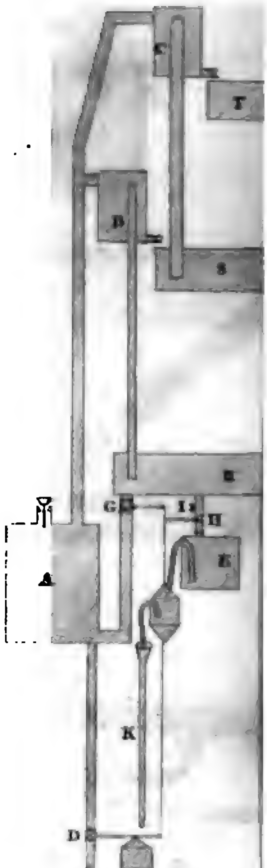
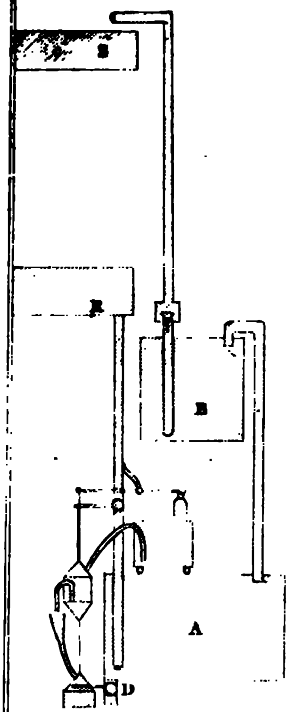


Fig. 1. at Schemnitz.



Mr. H. Sargeant's Machine for raising Water.

Stove of Cit. Guyton.

Fig. 1.

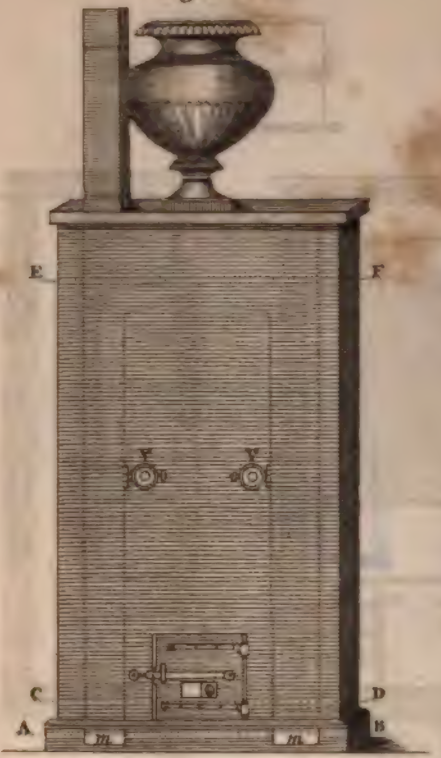
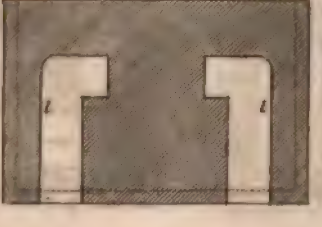
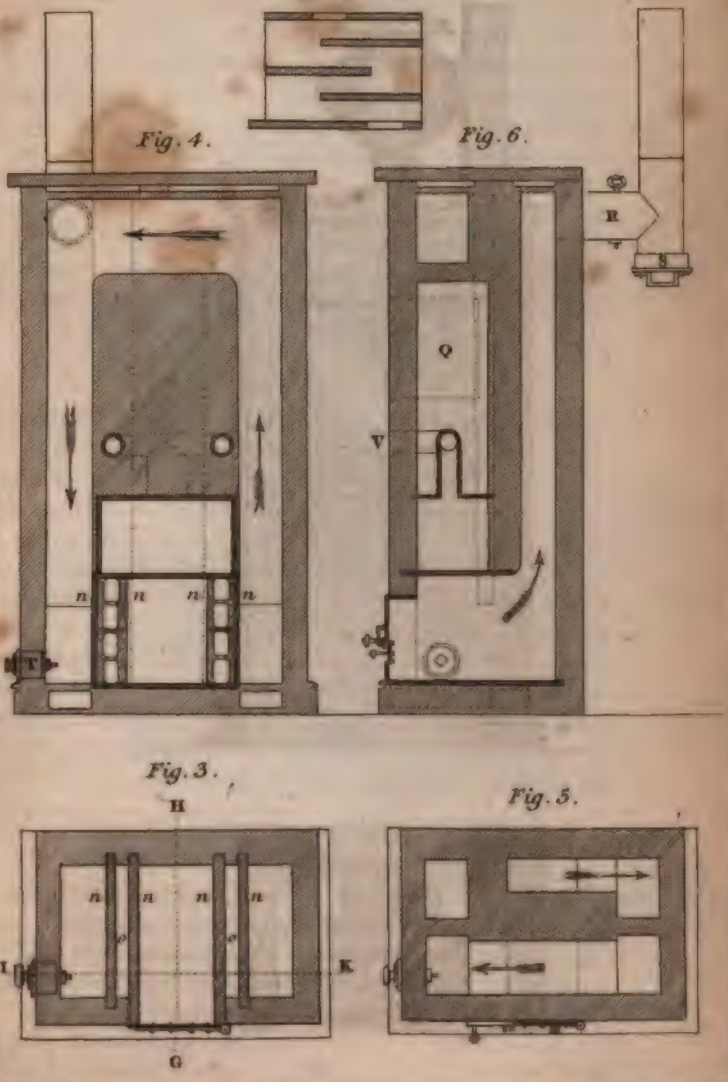


Fig. 2.



Section of the Stove of Guyton.

Fig. 7.



" ones? And is not this medium exceedingly more rare and " fubtile than the air, and exceedingly more elaftic and active? " And doth it not readily pervade all bodies? And is it not, " by its elaftic force, expanded through all the heavens?— " May not planets and comets, and all grofs bodies, perform " their motions in this ethereal medium? And may not its re- " fiftance be fo fmall, as to be inconfiderable? For inftance, if " this ether (for fo I will call it) fhould be fuppofed 700,000 " times more elaftic than our air, and above 700,000 times " more rare, its refiftance would be about 600,000,000 lefs " than that of water. And fo fmall a refiftance would fcarce " make any fenfible alteration in the motions of the planets, in " ten thoufand years. If any one would afk how a medium " can be fo rare, let him tell me, How an electric body can by " friction emit an exhalation fo rare and fubtile, and yet fo " potent? And how the effluvia of a magnet can pafs through " a plate of glafs without refiftance, and yet turn a magnetic " needle beyond the glafs?" (Optics, Qu. 18, 22.)

HYPOTHESIS II.

Undulations are excited in this Ether whenever a body becomes luminous.

Scholium. I ufe the word undulation, in preference to vibration, becaufe vibration is generally underftood as implying a motion which is continued alternately backwards and forwards, by a combination of the momentum of the body with an accelerating force, and which is naturally more or lefs permanent; but an undulation is fuppofed to confift in a vibratory motion, tranfmitted fucceffively through different parts of a medium, without any tendency in each particle to continue its motion, except in confequence of the tranfmiffion of fucceeding undulations, from a diftinct vibrating body; as, in the air, the vibrations of a chord produce the undulations conftituting found.

Hypoth. II. Light confifts in ethereal undulation.

Paffages from Newton.

" Were I to affume an hypothefis, it fhould be this, if propounded more generally, fo as not to determine what light " is, further than that it is fomething or other capable of exciting vibrations in the ether; for thus it will become fo ge- " neral and comprehenfive of other hypothefes, as to leave " little room for new ones to be invented." (Birch. Vol. III. p. 249. Dec. 1675.)

Detail of the hypoth. by Newton. Light.

"In the fecond place, it is to be fuppofed, that the ether is "a vibrating medium like air, only the vibrations far more "fwift and minute; thofe of air, made by a man's ordinary "voice, fucceeding one another at more than half a foot, or a "foot diftance; but thofe of ether at a lefs diftance than the "hundred thoufandth part of an inch. And, as in air the vi-"brations are fome larger than others, but yet all equally fwift, "(for in a ring of bells the found of every tone is heard at two "or three miles diftance, in the fame order that the bells are "ftruck), fo, I fuppofe, the ethereal vibrations differ in big-"nefs, but not in fwiftnefs. Now, thefe vibrations, befide "their ufe in reflection and refraction, may be fuppofed the "chief means by which the parts of fermenting or putrifying "fubftances, fluid liquors, or melted, burning, or other hot "bodies, continue in motion." (Birch. Vol. III. p. 251. Dec. 1675).

reflection, ...tion;

"When a ray of light falls upon the furface of any pellucid "body, and is there refracted or reflected, may not waves of "vibrations, or tremors, be thereby excited in the refracting "or reflecting medium? And are not thefe vibrations propa-"gated from the point of incidence to great diftances? And "do they not overtake the rays of light, and by overtaking "them fucceffively, do not they put them into the fits of eafy "reflection and eafy tranfmiffion defcribed above?" (Optics, Qu. 17).

and the alternate fits.

"Light is in fits of eafy reflection and eafy tranfmiffion, be-"fore its incidence on tranfparent bodies. And probably it is "put into fuch fits at its firft emiffion from luminous bodies, "and continues in them during all its progrefs." (Optics, Second Book, Part III. Prop. 13.)

HYPOTHESIS III.

The Senfation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.

Paffages from Newton.

Hypoth. III. Colour depends on the frequency of the ethereal vibrations.

"The objector's hypothefis, as to the fundamental part of it, is not againft me. That fundamental fuppofition is, that the parts of bodies, when brifkly agitated, do excite vibrations in the ether, which are propagated every way from thofe bodies in ftraight lines, and caufe a fenfation of light by beating and

and dafhing againft the bottom of the eye, fomething after the manner that vibrations in the air caufe a fenfation of found by beating againft the organs of hearing. Now, the moft free and natural application of this hypothefis to the folution of phenomena, I take to be this: that the agitated parts of bodies, according to their feveral fizes, figures, and motions, do excite vibrations in the ether of various depths or bigneffes, which, being promifcuoufly propagated through that medium to our eyes, effect in us a fenfation of light of a white colour; but if by any means thofe of unequal bigneffes be feparated from one another, the largeft beget a fenfation of a red colour, the leaft or fhorteft of a deep violet, and the intermediate ones of intermediate colours; much after the mannner that bodies, according to their feveral fizes, fhapes, and motions, excite vibrations in the air of various bigneffes, which, according to thofe bigneffes, make feveral tones in found: that the largeft vibrations are beft able to overcome the refiftance of a refracting fuperficies, and fo break through it with leaft refraction; whence the vibrations of feveral bigneffes, that is, the rays of feveral colours, which are blended together in light, muft be parted from one another by refraction, and fo caufe the phenomena of prifms, and other refracting fubftances; and that it depends on the thicknefs of a thin tranfparent plate or bubble, whether a vibration fhall be reflected at its further fuperficies, or tranfmitted; fo that, according to the number of vibrations, interceding the two fuperficies, they may be reflected or tranfmitted for many fucceffive thickneffes. And, fince the vibrations which make blue and violet, are fuppofed fhorter than thofe which make red and yellow, they muft be reflected at a lefs thicknefs of the plate: which is fufficient to explicate all the ordinary phenomena of thofe plates or bubbles, and alfo of all natural bodies, whofe parts are like fo many fragments of fuch plates. Thefe feem to be the moft plain, genuine, and neceffary conditions of this hypothefis. And they agree fo juftly with my theory, that if the animadverfor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Tranf. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

Particular developement; as in found. Various refrangibility: Thin tranfparent plates.

"To explain colours, I fuppofe, that as bodies of various fizes, denfities, or fenfations, do by percuffion or other action excite

Repetition of the theory.

excite founds of various tones, and confequently vibrations in the air of different bignefs; fo the rays of light, by impinging on the ftiff refracting fuperficies, excite vibrations in the ether, of various bignefs; the biggeft, ftrongeft, or moft potent rays, the largeft vibrations; and others fhorter, according to their bignefs, ftrength, or power: and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being fuch refracting fuperficies, when the rays impinge upon them, they muft there excite thefe vibrations, which vibrations (like thofe of found in a trunk or trumpet) will run along the aqueous pores or cryftalline pith of the capillamenta, through the optic nerves, into the fenforium; and there, I fuppofe, affect the fenfe with various colours, according to their bignefs and mixture; the biggeft with the ftrongeft colours, reds and yellows; the leaft with the weakeft, blues and violets; the middle with green; and a confufion of all with white, much after the manner that, in the fenfe of hearing, nature makes ufe of aerial vibrations of feveral bigneffes, to generate founds of divers tones; for the analogy of nature is to be obferved." (Birch, Vol. III. p. 262, Dec. 1675.)

"Confidering the laftingnefs of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the moft refrangible rays excite the fhorteft vibrations, —the leaft refrangible the largeft? May not the harmony and difcord of colours arife from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and difcord of founds arife from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. The parts of the retina being probably capable of vibrating in unifon with a limited number of colorific motions,

Scholium. Since, for the reafon here affigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations muft be dependent on the conftitution of this fubftance. Now, as it is almoft impoffible to conceive each fenfitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unifon with every poffible undulation, it becomes neceffary to fuppofe the number limited, for inftance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion lefs or more forcibly, by undula-

tions

tions differing lefs or more from a perfect unifon; for inftance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unifon with yellow and blue, and produce the fame effect as a light compofed of thofe two fpecies: and each fenfitive filament of the nerve may confift of three portions, one for each principal colour. Allowing this ftatement, it appears that any attempt to produce a mufical effect from colours, muft be unfuccefsful, or at leaft that nothing more than a very fimple melody could be imitated by them; for the period, which in fact conftitutes the harmony of any concord, being a multiple of the periods of the fingle undulations, would in this cafe be wholly without the limits of fympathy of the retina, and would lofe its effect; in the fame manner as the harmony of a third or a fourth is deftroyed, by depreffing it to the loweft notes of the audible fcale. In hearing, there feems to be no permanent vibration of any part of the organ.

It is not to be expected that colours can have a mufical effect.

The ear not permanently affected.

HYPOTHESIS IV.

All material Bodies have an attraction for the ethereal Medium, by means of which it is accumulated within their fubftance, and for a fmall Diftance around them, in a State of greater Denfity, but not of greater Elafticity.

Hypoth. IV. The denfity but not the elafticity of the ether, is greater within and near other bodies.

It has been fhewn, that the three former hypothefes, which may be called effential, are literally parts of the more complicated Newtonian fyftem. This fourth hypothefis differs perhaps in fome degree from any that have been propofed by former authors, and is diametrically oppofite to that of Newton; but, both being in themfelves equally probable, the oppofition is merely accidental; and it is only to be inquired which is the beft capable of explaining the phenomena. Other fuppofitions might perhaps be fubftituted for this, and therefore I do not confider it as fundamental, yet it appears to be the fimpleft and beft of any that have occurred to me.

Newton fuppofed the contrary.

PROPOSITION I.

All impulfes are propagated in a homogeneous elaftic Medium with an equable velocity.

Propof. I. Impulfe is propagated uniformly in an homogeneous elaftic medium.

Every experiment relative to found coincides with the obfervation already quoted from Newton, that all undulations are propagated through the air with equal velocity; and this is fur-

ther confirmed by calculations. (Lagrange, Mifc. Taur. Vol. I. p. 91. Alfo, much more concifely, in my Syllabus of a courfe of Lectures on Natural and Experimental Philofophy, about to be publifhed. Article 289.) If the impulfe be fo great as materially to difturb the denfity of the medium, it will be no longer homogeneous; but, as far as concerns our fenfes, the quantity of motion may be confidered as infinitely fmall. It is furprifing that Euler, although aware of the matter of fact, fhould ftill have maintained, that the more frequent undulations are more rapidly propagated. (Theor. muf. and Conject. phys.) It is poffible, that the actual velocity of the particles of luminiferous ether may bear a much lefs proportion to the velocity of the undulations than in found; for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves an hundred millions.

Law of the velocities in different mediums.

Scholium 1. It has been demonftrated, that in different mediums the velocity varies in the fubduplicate ratio of the force directly, and of the denfity inverfely. (Mifc. Taur. Vol. I. p. 91. Young's Syllabus. Art. 294.)

Undulations do not mix.

Scholium 2. It is obvious, from the phenomena of elaftic bodies and of founds, that the undulations may crofs each other without interruption. But there is no neceffity that the various colours of white light fhould intermix their undulations; for, fuppofing the vibrations of the retina to continue but a five hundredth of a fecond after their excitement, a million undulations of each of a million colours may arrive in diftinct fucceffion within this interval of time, and produce the fame fenfible effect, as if all the colours arrived precifely at the fame inftant.

PROPOSITION II.

Prop. II. Nature of undulation.

An Undulation conceived to originate from the Vibration of a fingle Particle, muft expand through a homogeneous Medium in a fpherical Form, but with different Quantities of Motion in different Parts.

For, fince every impulfe, confidered as pofitive or negative, is propagated with a conftant velocity, each part of the undulation muft in equal times have paft through equal diftances from the vibrating point. And, fuppofing the vibrating particle, in the courfe of its motion, to proceed forwards to a fmall diftance in a given direction, the principal ftrength of the undulation

ation will naturally be ftraight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evanefcent.

Now, in order that fuch an undulation may continue its progrefs to any confiderable diftance, there muft be in each part of it, a tendency to preferve its own motion in a right line from the centre; for, if the excefs of force at any part were communicated to the neighbouring particles, there can be no reafon why it fhould not very foon be equalifed throughout, or, in other words, become wholly extinct, fince the motions in contrary directions would naturally deftroy each other. The origin of found from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the fame inftant either elevated or depreffed. It may be difficult to fhow mathematically, the mode in which this inequality of force is preferved; but the inference from the matter of fact, appears to be unavoidable; and, while the fcience of hydrodynamics is fo imperfect that we cannot even folve the fimple problem of the time required to empty a veffel by a given aperture, it cannot be expected that we fhould be able to account perfectly for fo complicated a feries of phenomena, as thofe of elaftic fluids. The theory of Huygens indeed explains the circumftance in a manner tolerably fatisfactory: he fuppofes every particle of the medium to propagate a diftinct undulation in all directions; and that the general effect is only perceptible where a portion of each undulation confpires in direction at the fame inftant; and it is eafy to fhow that fuch a general undulation would in all cafes proceed rectilinearly, with proportionate force; but, upon this fuppofition, it feems to follow, that a greater quantity of force muft be loft by the divergence of the partial undulations, than appears to be confiftent with the propagation of the effect to any confiderable diftance. Yet it is obvious, that fome fuch limitation of the motion muft naturally be expected to take place; for, if the intenfity of the motion of any particular part, inftead of continuing to be propagated ftraight forwards, were fuppofed to affect the intenfity of a neighbouring part of the undulation, an impulfe muft then have travelled from an internal to an external circle in an oblique direction, in the fame time as in the direction of the radius, and confequently with a greater velocity; againft the firft propofition.

Theory of Huygens.

propofition. In the cafe of water, the velocity is by no means fo rigidly limited as in that of an elaftic medium. Yet it is not neceffary to fuppofe, nor is it indeed probable, that there is abfolutely not the leaft lateral communication of the force of the undulation, but that, in highly elaftic mediums, this communication is almoft infenfible. In the air, if a chord be perfectly infulated, fo as to propagate exactly fuch vibrations as have been defcribed, they will in fact be much lefs forcible than if the chord be placed in the neighbourhood of a founding board, and probably in fome meafure becaufe of this lateral communication of motions of an oppofite tendency. And the different intenfity of different parts of the fame circular undulation may be obferved, by holding a common tuning fork at arm's length, while founding, and turning it, from a plane directed to the ear, into a pofition perpendicular to that plane.

PROPOSITION III.

Propofition III. Lateral undulations explained.

A Portion of a fpherical Undulation, admitted through an Aperture into a quiefcent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the inftant of admiffion, the circumference of each of the undulations may be fuppofed to generate a partial undulation, filling up the nafcent angle between the radii and the furface terminating the medium; but no fenfible addition will be made to its ftrength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with refpect to the various force of a fpherical undulation. If indeed the aperture bear but a fmall proportion to the breadth of an undulation, the newly generated undulation may nearly abforb the whole force of the portion admitted; and this is the cafe confidered by Newton in the Principia. But no experiment can be made under thefe circumftances with light, on account of the minutenefs of its undulations, and the interference of inflection; and yet fome faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture diftinctly vifible in all directions; thefe are attributed by Newton to fome unknown caufe, diftinct from inflection: (Optics, Third

Third Book, Obf. 5. and they fully anfwer the defcription of this propofition.

Let the concentric lines in Fig. 1. (Plate V.) reprefent the contemporaneous fituation of fimilar parts of a number of fucceffive undulations diverging from the point A; they will alfo reprefent the fucceffive fituations of each individual undulation: let the force of each undulation be reprefented by the breadth of the line, and let the cone of light A B C be admitted through the aperture B C; then the principal undulations will proceed in a rectilinear direction towards G H, and the faint radiations on each fide will diverge from B and C as centres, without receiving any additional force from any intermediate point D of the undulation, on account of the inequality of the lines D E and D F. But, if we allow fome little lateral divergence from the extremities of the undulations, it muft diminifh their force, without adding materially to that of the diffipated light; and their termination, inftead of the right line B G, will affume the form C H; fince the lofs of force muft be more confiderable near to C than at greater diftances. This line correfponds with the boundary of the fhadow in Newton's firft obfervation, Fig. 1; and it is much more probable that fuch a diffipation of light was the caufe of the increafe of the fhadow in that obfervation, than that it was owing to the action of the inflecting atmofphere, which muft have extended a thirtieth of an inch each way in order to produce it; efpecially when it is confidered that the fhadow was not diminifhed by furrounding the air with a denfer medium than air, which muft in all probability have weakened and contracted its inflecting atmofphere. In other circumftances, the lateral divergence might appear to increafe, inftead of diminifhing, the breadth of the beam.

Linear explanation.

As the fubject of this propofition has always been efteemed the moft difficult part of the undulatory fyftem, it will be proper to examine here the objections which Newton has grounded upon it.

Objections of Newton to the undulatory fyftem of light.

"To me, the fundamental fuppofition itfelf feems impoffible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in ftraight lines, without a continual and very extravagant fpreading and bending every way into the quiefcent medium, where they are terminated by it.

it. I miftake, if there be not both experiment and demonftration to the contrary." (Phil. Tranf. VII. 5089, Abr. I. 146. Nov. 1672.)

"*Motus omnis per fluidum propagatus divergit a recto tramite in fpatia immota.*"

"*Quoniam medium ibi,*" in the middle of an undulation admitted, "*denfius eft, quam in fpatiis hinc inde, dilatabit fefe tam verfus fpatia utrinque fita, quam verfus pulfuum rariora intervalla; eoque pacto—pulfus eadem fere celeritate fefe in medii partes quiefcentes hinc inde relaxare debent;—ideoque fpatium totum occupabunt.—Hoc experimur in fonis.*" (Princip. Lib. II. Prop. 42.)

"Are not all hypothefes erroneous, in which light is fuppofed to confift in preffion or motion, propagated through a fluid medium?—If it confifted in preffion or motion, propagated either in an inftant, or in time, it would bend into the fhadow. For preffion or motion cannot be propagated in a fluid in right lines beyond an obftacle which ftops part of the motion, but will bend and fpread every way into the quiefcent medium which lies beyond the obftacle. The waves on the furface of ftagnating water, paffing by the fides of a broad obftacle which ftops part of them, bend afterwards, and dilate themfelves gradually into the quiet water behind the obftacle. The waves, pulfes, or vibrations of the air, wherein founds confift, bend manifeftly, though not fo much as the waves of water. For a bell or a cannon may be heard beyond a hill, which intercepts the fight of the founding body; and founds are propagated as readily through crooked pipes as ftraight ones. But light is never known to follow crooked paffages, nor to bend into the fhadow. For the fixed ftars, by the interpofition of any of the planets, ceafe to be feen. And fo do the parts of the fun, by the interpofition of the moon, Mercury, or Venus. The rays which pafs very near to the edges of any body, are bent a little by the action of the body; but this bending is not towards but from the fhadow, and is performed only in the paffage of the ray by the body, and at a very fmall diftance from it. So foon as the ray is paft the body, it goes right on." (Optics, Qu. 28.)

Anfwers to the objections of Newton.

Now the propofition quoted from the Principia does not directly contradict this propofition; for it does not affert that fuch a motion muft diverge equally in all directions; neither can

can it with truth be maintained, that the parts of an elaſtic medium communicating any motion, muſt propagate that motion equally in all directions. (Phil. Tranſ. for 1800. p. 109—112.) All that can be inferred by reaſoning is, that the marginal parts of the undulation muſt be ſomewhat weakened, and that there muſt be a faint divergence in every direction; but whether either of theſe effects might be of ſufficient magnitude to be ſenſible, could not have been inferred from argument, if the affirmative had not been rendered probable by experiment.

Sound deflects leſs than waves of water.

As to the analogy with other fluids, the moſt natural inference from it is this: "The waves of the air, wherein ſounds "conſiſt, bend manifeſtly, though not ſo much as the waves "of water;" water being an inelaſtic, and air a moderately elaſtic medium; but ether being moſt highly elaſtic, its waves bend very far leſs than thoſe of the air, and therefore almoſt imperceptibly. Sounds are propagated through crooked paſſages, becauſe their ſides are capable of reflecting ſound, juſt as light would be propagated through a bent tube, if perfectly poliſhed within.

Crooked paſſages.

The light of a ſtar is by far too weak to produce, by its faint divergence, any viſible illumination of the margin o a planet eclipſing it; and the interception of the ſun's light by the moon, is as foreign to the qneſtion, as the ſtatement of inflection is inaccurate.

To the argument adduced by Huygens, in favour of the rectilinear propagation of undulations, Newton has made no reply; perhaps becauſe of his own miſconception of the nature of the motions of elaſtic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians. (Phil. Tranſ. for 1800, p. 116.) On the whole, it is preſumed, that this propoſition may be ſafely admitted, as perfectly conſiſtent with analogy and with experiment.

(To be continued.)

VIII. *Remarks*

VIII.

Remarks on Combuftion. By THOMAS THOMSON, M. D. Lecturer on Chemiftry in Edinburgh.

(Concluded from Page 20.)

Nitric acid formed fpontaneoufly;

VI. NITRIC acid is formed fpontaneoufly upon the furface of the earth by proceffes with which we are but imperfectly acquainted; but which certainly have no refemblance to combuftion. Its oxigen is probably furnifhed by the *air*, which is a fupporter; at leaft, it has been obferved, that if azote, the inftant it is evolved, comes in contact with air, it is capable of combining with its oxigen, and forming nitric acid.

and alfo by electricity, through common air, and probably by galvanifm:

Nitric acid may be formed alfo, as Mr. Cavendifh has demonftrated, by paffing electric fparks through common air, a fupporter. In all probability it may be formed alfo by the galvanic pile, but this may be confidered as equivalent to electricity. This formation of nitric acid by means of electricity, has been confidered as a combuftion, but for what reafon it is not eafy to fay: the fubftance acted upon is not a combuftible with a fupporter, but a fupporter alone. Electricity is fo far from being equivalent to combuftion, that it fometimes acts in a manner diametrically oppofite; *unburning*, if I may ufe the expreffion, a fubftance which has already undergone combuftion, and converting a *product* into a *combuftible* and a *fupporter*. Thus it decompofes water, and converts it into oxigen and hidrogen gas; therefore it muft be capable of fupplying the fubftances which the oxigen and combuftible lofe when they combine by combuftion, and form a product *.

Apparently by decombuftion.

Nitric acid in the procefs for compofing water.

There is one procefs more, during which nitric acid is formed, which muft at firft fight appear an exception to the general rule; I mean the formation of nitric acid, which takes place during the combuftion of hidrogen gas in oxigen gas contaminated with air. But in this cafe it is the *hidrogen* only which burns, and not the *air*; the air indeed combines intimately, and forms nitric acid, juft as it does when electric fparks are

* I do not mean to affirm that electricity never occafions combuftion, the contrary of which is well known, but that a combination produced by it is not always the fame with combuftion.

paffed

paffed through it. But this procefs has no refemblance to combuftion. We fee, however, that a certain temperature is capable of producing this change in air.

8. Several of the fupporters and partial fupporters are capable of combining with combuftibles, without undergoing decompofition, or exhibiting the phenomena of combuftion. In this manner the yellow oxide of gold and the white oxide of filver combine with ammonia; the red oxide of mercury with oxalic acid; and oximuriatic acid with ammonia. Thus alfo nitre and oximuriate of potafh may be combined, or at leaft intimately mixed with feveral combuftible bodies, as in gunpowder, &c. In all thefe compounds the oxigen of the fupporter and the combuftible retain the ingredients which render them fufceptible of combuftion; hence the compound is ftill combuftible: And in confequence of the intimate combination of the component parts, the leaft alteration is apt to deftroy the equilibrium which fubfifts between them; the confequence is, combuftion and the formation of a new compound. Hence thefe compounds burn with amazing facility, not only when heated, but when triturated or ftruck fmartly with a hammer. They have therefore received the name of *detonating* or *fulminating* bodies. Thus we have fulminating gold, fulminating filver, fulminating mercury, fulminating powder, &c.

Supporters, &c. may combine with combuftibles without combuftion, and produce *fulminating compounds*.

9. Such are the properties of the combuftibles, the fupporters, and the products; and fuch the phenomena which they exhibit when made to act upon each other.

If we compare together the *fupporters* and the *products*, we fhall find that they refemble each other in feveral refpects. Both of them contain oxigen as an effential conftituent part; both are capable of converting combuftibles into products; and feveral of both combine with combuftibles and with additional dofes of oxigen. But they differ widely from each other in the phenomena which accompany their action on combuftibles. The fupporters convert thefe bodies into products, and combuftion, or the emiffion of heat and light at the fame time, takes place; whereas the products convert combuftibles into products without any fuch emiffion. Now, as the ultimate change produced upon combuftibles by both thefe fets of bodies is the fame, and as the fubftance which combines with the combuftibles is in both cafes the fame, namely oxigen, we

Supporters and products refemble in many refpects;

but they differ widely in their effect on combuftibles. The former only produce combuftion.

must conclude that this oxigen in the supporters contains something which the oxigen of the products wants, something which separates during the passage of the oxigen from the product to the combustible, and occasions the combustion, or emission of fire, which accompanies this passage. The oxigen of supporters then contains some ingredient which the oxigen of products wants. Many circumstances concur to render it probable that this ingredient is *caloric*.

The oxigen of *supporters* contains *caloric*.

The *combustibles* and the *products* also resemble each other in several respects. Both of them contain the same or a similar base; both frequently combine with combustibles, and likewise with oxigen; but they differ essentially in the phenomena which accompany their combination with oxigen. In the one case *fire* is emitted, in the other not. If we recollect that no substance but a combustible is capable of restoring combustibility to the base of a product, and that at its doing so it always loses its own combustibility; and if we recollect farther, that the base of a product does not exhibit the phenomena of combustion even when it combines with oxigen, we cannot avoid concluding, that all combustibles contain an ingredient which they lose when converted into products and that this loss contributes to the fire which makes its appearance during the conversion. Many circumstances contribute to render it probable that this ingredient is *light*.

Combustibles and products also resemble each other: But the former *emit fire* when they combine with oxigen.

Combustibles contain *light*.

If we suppose that the oxigen of supporters contains caloric as an essential ingredient, and that light is a component part of all combustibles, the phenomena of combustion above enumerated, numerous and intricate as they are, admit of an easy and obvious explanation. The component parts of the oxigen of supporters are two; namely, 1. a base, 2. caloric: The component parts of combustibles are likewise two; namely, 1. a base, 2. light. During combustion the base of the oxigen combines with the base of the combustible, and forms the product; while at the same time the caloric of the oxigen combines with the light of the combustible, and the compound flies off in the form of fire. Thus combustion is a double decomposition; the oxigen and combustible divide themselves each into two portions, which combine in pairs; the one compound is the *product*, and the other the *fire*, which escapes.

When supporters and combustibles combine, the *caloric and light* fly off in the *combination called fire*.

Hence the reason that the oxigen of products is unfit for combustion. It wants its caloric. Hence the reason that combustion

Combustion will not ensue if either of these in-

tion does not take place when oxigen combines with products or with the base of supporters. These bodies contain no light. The caloric of the oxigen of course is not separated, and no fire appears. And this oxigen still retaining its caloric, is capable of producing combustion whenever a body is presented which contains light, and whose base has an affinity for oxigen. Hence also the reason why a combustible alone can restore combustibility to the base of a product. In all such cases a double decomposition takes place. The oxigen of the product combines with the base of the combustible, while the light of the combustible combines with the base of the product. Thus when iron acts on water, the oxigen of the water combines with the base of the iron, while at the same time the *light* of the iron combines with the hidrogen of the water, and occasions its escape in the state of gas.

gredients of fire be absent.

But the application of this theory to all the different phenomena described above, is so obvious, that it is needless to give any more examples. Let us rather inquire into the evidences which can be brought forward in its support.

10. Now as caloric and light are always emitted during combustion, it follows that they must have been previously component parts either of the combustible, or of the supporter, or of both.

They must therefore have previously existed in the combustible, the supporter, or both.

That the oxigen of the supporters contains either one or both of these substances, follows incontrovertibly from a fact already mentioned, namely, that the oxigen of products will not support combustion, while that of supporters will. Hence the oxigen of supporters must contain something which the oxigen of products wants, and this something must be caloric, or light, or both.

That the oxigen of some of the supporters at least contains caloric as an ingredient, has been proved, I think, in a satisfactory manner, by the experiments of Crawford, Lavoisier, and La Place. Thus the temperature of hot blooded animals is maintained by the decomposition of *air*. Now if the oxigen of one supporter contain caloric, the same ingredient must exist in the oxigen of every supporter, because all of them are obviously in the same state. Hence I conclude that the oxigen of every supporter contains caloric as an essential ingredient.

References to facts: Caloric.

The light emitted during combustion must either proceed from the combustible or the supporter. Now that it proceeds from the combustible must appear pretty obvious, if we recollect

Light.

left that the colour of the light emitted during combuſtion varies, and that this variation uſually depends, not upon the ſupporter, but upon the combuſtible. Thus carbonic acid burns with a blue flame, carbonated hidrogen with a white, and charcoal with a red; ſulphur with a blue or violet, zinc with a greeniſh white, and phoſphorus with a white.

Natural formation of combuſtibles and ſupporters: Vegetation.

The formation of combuſtibles in plants obviouſly requires the preſence and agency of light; for when plants vegetate in the dark, their carbon is not increaſed, nor is any oily or reſinous matter formed in them. The leaves of plants emit oxigen gas when expoſed to the ſun's rays, but never in the ſhade, or in the dark. Senebier has demonſtrated that this emiſſion is occaſioned by the decompoſition of carbonic acid. This acid, which is a product of combuſtion, is decompoſed by the leaves of plants aſſiſted by ſunſhine, and converted into oxigen gas and charcoal, a *ſupporter* and a *combuſtible*. This proceſs is exactly the reverſe of combuſtion, and muſt therefore reſtore the ſubſtances which had been loſt during combuſtion; that is to ſay, caloric and light. But the ſun's rays conſiſt of theſe two bodies. Thus we ſee why plants require ſunſhine. A part of vegetation conſiſts in decompoſing, or *unburning*, products, and converting them into ſupporters and combuſtibles; but for ſuch a converſion caloric and light are abſolutely neceſſary.

The ſame effects by art.

Beſides vegetation, we are acquainted with two other methods of unburning products, or of converting them into products and combuſtible; by expoſing them, in certain circumſtances, to the agency of *fire* or of *electricity*. The oxides of gold, ſilver, and mercury, when heated to redneſs, are decompoſed, oxigen gas is emitted, and the pure metal remains behind. In this caſe the neceſſary caloric and light muſt be furniſhed by the fire; a circumſtance which explains why ſuch reductions always require a red heat. When carbonic acid is made to paſs repeatedly over red-hot charcoal, it combines with a portion of charcoal, and is converted into carbonic oxide gas. If this gas be a combuſtible oxide, the baſe of the carbonic acid and its oxigen muſt have been ſupplied with light and caloric from the fire; but if it be a *partial combuſtible*, it is merely a compound of carbonic acid and charcoal: which of the two it is, remains ſtill to be aſcertained. Electricity decompoſes water, and converts it into oxigen gas and hidrogen gas; it muſt therefore ſupply the heat and the light which theſe bodies loſt when converted into a product.

Theſe

These facts, together with the exact correspondence of the theory given above with the phenomena of combustion, render it so probable, that I have ventured to propose it as an additional step towards a full explanation of the theory of combustion. Every additional experiment has served to confirm it more and more *. It even throws light upon many phenomena which have been hitherto considered as altogether anomalous, as will be evident from the following observations.

Application of this theory to other facts.

II. In the year 1793, the associated Dutch chemists drew the attention of philosophers to a curious phenomenon which accompanies the formation of some of the sulphurets; a phenomenon previously noticed by Scheele; but which they first described in detail. When eight parts of copper filings, and three parts of flowers of sulphur are mixed together in a glass receiver, and the vessel placed upon burning coals, the mixture melts, a kind of explosion takes place, it becomes suddenly red hot, and a glow, like that of a piece of red hot charcoal fanned by bellows, rapidly pervades the whole. When this disappears, the mixture is found in the state of solid sulphuret of copper. Iron, lead, tin or zinc, may be substituted for copper. The experiment succeeds whether the vessel be filled with air, or with azotic, or hidrogen gas, or even with water or mercury. What is singular in this experiment is the glowing *red heat*, or the emission of *fire* which accompanies the combination of the sulphur and metal. This emission being the same which takes place during combustion, the process has been considered as a combustion, and stated as such by the German chemists, as an objection to Lavoisier's theory, which supposes that oxigen is a necessary agent in that process: while other philosophers have denied that this operation is a combustion, or that it has any resemblance to that process.

Ignition produced by the fusion of sulphur with a metal,

The same emission of caloric and light, or of *fire*, takes place when melted sulphur is made to combine with pot-ash, or with lime, in a crucible or glass tube, and likewise when

or of sulphur with potash or lime; or phosphorus with lime, &c.

* In the preceding enumeration of facts I have not taken notice of the modifications which the Lavoiserian theory has received from Hutton, Delametherie, Richter, and Brugnatelli; because I suppose them sufficiently known. Every one of these modifications agrees in some particulars with the theory given in this Paper, but differs from it in others.

 melted

melted phoſphorus is made to combine with lime heated nearly to redneſs. In all probability barytes and ſtrontian exhibit the ſame phenomenon when combined with melted ſulphur or phoſphorus; and ſome of the metals when combined with phoſphorus. In general then the emiſſion of *fire* accompanies the combination of melted ſulphur and phoſphorus, with ſeveral of the earths, fixed alkalies and metals, heated previouſly to a certain temperature.

Explanation. *Fuſed* ſulphur or phoſphorus contain *caloric*: metal. &c. contain *light*. They become *ſolid* in combination and give out *caloric* and *light*; i. e. fire.

To explain the phenomenon we have only to recollect, 1. That the ſulphur and phoſphorus are in the melted ſtate, and therefore contain caloric as an ingredient. 2. That the alkalies, earths and metals which produce the phenomenon in queſtion, contain light as an eſſential ingredient. 3. That the ſulphuret or phoſphuret formed is always in a ſolid ſtate; theſe three points once eſtabliſhed, the proceſs admits of a very ſimple explanation. The ſulphur or phoſphorus combines with the baſe of the metal, earth or alkali; while at the ſame time the *caloric* to which the ſulphur or phoſphorus owed its fluidity, combines with the *light* of the metal, earth, or alkali, and the compound flies off under the form of *fire*.

The proceſs reſembles combuſtion except in its product;

Thus the proceſs is exactly the ſame with combuſtion, excepting as far as regards the product. The melted ſulphur or phoſphorus acts the part of the *ſupporter*, while the metal, earth or alkali occupy the place of the *combuſtible*. The firſt furniſhes caloric, the ſecond light, while the baſe of each combines together. Hence we ſee that the baſe of ſulphurets and phoſphurets reſembles the baſe of products in being deſtitute of light, the formation of theſe bodies exhibiting the ſeparation of fire like *combuſtion*, but the product differing from a product of combuſtion in being deſtitute of oxigen, we may diſtinguiſh the proceſs by the title of *ſemi-combuſtion*; indicating by the term, that it poſſeſſes one half of the characteriſtic marks of combuſtion, but is deſtitute of the other half.

termed ſemi-combuſtion.

Facts to ſhew that potaſh and lime contain light.

The only part of this theory which requires proof is, that light is a component part of the earths and alkalies. But as potaſh and lime are the only bodies of that nature, which I am certain to be capable of exhibiting the phenomena of ſemi-combuſtion, the proofs muſt of neceſſity be confined to them. Now that *lime* contains light as a component part has been long known. Meyer and Pelletier obſerved long ago, that when water is poured upon quicklime not only heat but light is emitted. Light is emitted alſo abundantly when ſulphuric acid

acid is poured upon lime*. In both cafes a *femi-combuftion* takes place. The water and the acid being folidified give out *caloric*, while the quicklime gives out *light*; that lime during its calcination combines with light, and that light is a component part of quicklime is demonftrated by the following experiment, for which we are indebted to Scheele.

Semi-combuftion of lime and water.

It is well known that fluor fpar (native fluate of lime) has the property of phofphorefcing ftrongly when heated, but that the experiment does not fucceed twice with the fame fpecimen. After it has been once heated fufficiently, no fubfequent heat will caufe it to phofphorate. Now phofphorefcence is merely the emiffion of light, light of courfe is a component part of fluor fpar, and heat has the property of feparating it. But the phofphorefcing quality of the fpar may be again recovered to it, or which is the fame thing, the light which the fpar had loft may be reftored by the following procefs. Decompofe the fluate of lime by fulphuric acid, and preferve the fluoric acid feparated. Boil the fulphate of lime thus formed with a fufficient quantity of carbonate of foda; a double decompofition takes place; fulphate of foda remains in folution, and carbonate of lime precipitates. Calcine this precipitate in a crucible till it is reduced to quicklime, and combine it with the fluoric acid to which it was formerly united. The fluor fpar thus regenerated phofphorefces as at firft. Hence the lime during its calcination muft have combined with light.

Phofphorefcence of fluor fpar explained,

reftored.

That potafh contains light, may be proved in the fame manner as the exiftence of that body in quicklime. Dizé has fhown that much light is emitted when fulphuric acid is poured upon potafh, but more when it is poured on the carbonate of potafh. Now as potafh is deprived of its carbonic acid by lime, it is obvious that the procefs muft be a double decompofition; the bafe of the lime combines with carbonic acid, while its light combines with the potafh.

Light in potafh.

Thefe remarks on femi-combuftion might eafily be extended much farther. For it is obvious, that whenever a liquid combines with a folid containing light, and the product is a folid body, fomething analogous to femi-combuftion muft take place. Hence the reafon why water increafes the violence of combuftion when thrown fparingly into a common fire.

* Dizé Jour. de Phyf. 49, 177.

IX.

ation of the Experiments and Obſervations on the Light ... is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on ſolar Light, when imbibed by CANTON'S *Phoſphorus.* *By* NATHANIEL HULME, *M.D. F.R.S. and A.S.*

(Concluded from page 40.)

§ 5.

*ts of carbonic Acid Gas or fixed Air * on ſpontaneous Light.*

EXPERIMENTS.

...ſpontaneous light; but it revives by common air.

1. AT 10 P.M. a piece of freſh herring, weighing ... three drams, was ſuſpended in a wide-mouthed ten-ounce phial, filled with carbonic acid gas, and cloſed with a cork and bladder. It was retained there for three ſucceſſive nights; but emitted no light.

Exp. 2. The ſame experiment was made with a piece of herring, which was beginning to be luminous. On the next evening, the illumination was found to be extinct: nevertheleſs the herring was ſtill kept in the gas for three nights longer, but did not become lucid.

Exp. 3. At 7 P. M. a piece of freſh mackerel was introduced above water, into a wide-mouthed bottle, holding 24 ounces, which was completely filled with carbonic acid gas, and ſupported by a tea-ſaucer that held about three ounces of water. On the ſecond night it was dark, and continued the ſame on the third. It was then expoſed to the influence of atmoſpherical air, and, on the next evening, it was pretty luminous, and likewiſe on the ſucceeding night.

Exp. 4. At 9 P. M. a cork, ſmeared with the luminous matter of a mackerel, was put into a five-ounce wide-mouthed phial, filled with carbonic acid gas, and then cloſed with a glaſs

* This gas was obtained from powdered chalk, or marble, and diluted ſulphuric acid.

ſtopple.

ftopple. It continued to ſhine pretty vividly for ſome little time; then the light gradually diminiſhed, ſo that at twelve, only a ſmall ſpark remained.

Exp. 5. At 10 P. M. another cork, illuminated with mackerel-light, was introduced above water, into 24 ounces of the gas; and its light was nearly extinct at twelve.

Exp. 6. At 8 P. M. a fragment of ſhining wood was put above water, into 24 ounces of the gas; and it had not been long there before the light diſappeared. It was then taken out, and expoſed to the action of atmoſpheric air, when its ſhining property ſoon returned.

Exp. 7. Another fragment of brightly ſhining wood was introduced above water, into the ſame quantity of the gas, at 10 P. M. and the light was extinguiſhed in the ſpace of an hour. After this, it was expoſed to the open air, and the light gradually revived.

Exp. 8. At 8 P. M. a luminous dead glow-worm was put above water into the gas; its glowing appearance gradually faded, and in a ſhort time became quite inviſible. It was then taken out, and the light, by degrees, re-appeared as vivid as before.

OBSERVATION.

This gas, we find, has alſo an extinguiſhing property, with reſpect to ſpontaneous light: but, in general, the light returns, if the object of experiment be taken out, and expoſed to the open air.

§ 6

The Effects of ſulphurated hidrogen Gas on ſpontaneous Light.*

EXPERIMENTS.

Sulphurated hidrogen extinguiſhes ſpontaneous light more effectually and more permanently than carbonic acid.

Exp. 1. At noon, a piece of a very freſh mackerel, with a bright eye, was introduced above water, into 24 ounces of this gas, and was retained therein for three ſucceſſive evenings, without emitting any light. It was then expoſed to atmoſpheric air; yet it continued dark on the two following nights;

* This gas was obtained from ſulphuret of potaſh and diluted muriatic acid.

nights: but, on the third, it was very luminous, and remained ſo on the fourth and fifth.

Exp. 2. The ſame experiment was then made with a piece of freſh herring, which was alſo kept in the above gas, for about three nights, without being luminous. After expoſure to common air, it did not emit any light during the firſt 24 hours. However, on the ſubſequent night, it began to ſhine, had a very bright light on the following evening, and continued ſhining for ſeveral ſucceeding nights.

Exp. 3. A cork, ſmeared with the luminous matter of a herring, was put above water, into 24 ounces of the gas; and the light was extinguiſhed in leſs than an hour. The experiment was repeated in the ſame gas, and with the ſame reſult.

Exp. 4. A cork, illuminated with mackerel light, was introduced into the ſame quantity of gas; and was dark in half an hour.

Exp. 5. A fragment of ſhining wood, being put into the gas, became dark in eight minutes. A ſecond piece became dark in five minutes. They were then taken out, and continued dark all that evening. On the next evening, one of the pieces was uncommonly lucid.

Exp. 6. At 10 P. M. another fragment of brightly ſhining wood was introduced above water, into 24 ounces of the gas, and was extinct at eleven. It was then expoſed to the open air; but there was no return of light that evening. On the following night, it was found pretty luminous.

Exp. 7. A finely ſhining dead glow-worm was next put above water, into this gas, and its light was quickly extinguiſhed. In a ſecond experiment, in the ſame gas, the light was much ſlower in its extinction. In both inſtances, after the inſect was withdrawn, and placed in atmoſpheric air, the light gradually revived.

OBSERVATION.

It is apparent, by theſe experiments, that ſulphurated hidrogen gas extinguiſhes ſpontaneous light much ſooner than carbonic acid gas, and that, in general, the light returns much more ſlowly, when the ſubject is expoſed to atmoſpheric air.

§ 7.

§ 7.

*The Effects of nitrous Gas * on ſpontaneous Light.*

EXPERIMENTS.

Nitrous gas quickly and effectually extinguiſhes ſpontaneous light.

Exp. 1. A piece of freſh herring was introduced above water, into this gas, at 5 P. M. and remained there four nights, without emitting any light: it was then withdrawn, and expoſed to common air, for the ſpace of three nights; but did not become lucid.

Exp. 2. The ſame experiment was made with a piece of herring beginning to be luminous; but its light was gradually extinguiſhed: it was detained in the gas for three nights, and taken out dark. It was then expoſed to the open air, for the three ſubſequent nights; but its ſhining appearance did not return.

Exp. 3. A cork with luminous matter, introduced above water, into this ſpecies of gas, had its light, in general, extinguiſhed in from 10 to 30 minutes; and, when taken into common air, its light very ſeldom re-appeared.

Exp. 4. Fragments of ſhining wood, above water, in nitrous gas, were likewiſe commonly rendered dark in a very ſhort ſpace of time, as in three or four minutes; ſometimes a fragment, if uncommonly luminous, would not be extinguiſhed in leſs than ſix or eight minutes; and very ſeldom would the light revive, on expoſing the wood to atmoſpherical air.

Exp. 5. A dead ſhining glow-worm being put above water, into this gas, its light was quickly extinquiſhed; but, after the inſect was taken into the common atmoſphere, the light gradually returned. The experiment was thrice repeated, and with the ſame reſult.

OBSERVATION.

This ſpecies of gas, we obſerve to have totally prevented the emiſſion of light, and to have quickly extinguiſhed that which had been emitted: likewiſe that the luminous objects which had been under its influence, (except the glow-worm) did not experience a revival of their light, when taken out, and kept for ſome time in common air.

* This gas was obtained from copper and diluted nitrous acid.

§ 8.

§ 8.

The Effects of a Vacuum on ſpontaneous Light.

EXPERIMENTS.

The light is extinguiſhed in vacuo, but beautifully reſtored by admiſſion of air.

Exp. 1. A piece of ſhining wood, of a moderate ſize, was put under the receiver of an air-pump, in a dark room; in proportion as the air was extracted, the light was gradually extinguiſhed, and at laſt reduced to a mere point, juſt viſible, owing moſt probably to a ſmall reſiduum of air, which is always left, even in the moſt perfect machine. Freſh air was then leiſurely admitted, and the light was immediately revived in a very beautiful manner. This experiment was frequently repeated, and always with the like effect.

Exp. 2. Some luminous matter of a herring, uncommonly bright, was ſmeared upon a piece of red blotting paper, and then ſubmitted to the operation of the air-pump. The light became fainter and fainter, as the incloſed air was withdrawn, and at laſt nearly vaniſhed; but brightened up as before, on the influx of freſh air. The experiment was repeated, and with the ſame reſult.

SECTION XII.

Experiments and Obſervations on ſolar Light, when imbibed by Canton's Phoſphorus.

§ 1.

The Effects of Heat on imbibed ſolar Light.

I. *The imbibed Light is rendered more vivid by a moderate Degree of Heat.*

EXPERIMENTS.

Canton's phoſphorus ſhines more when heated.

Exp. 1. Having prepared ſome Canton's phoſphorus, and expoſed it to the light of the ſun, it was carried into the dark laboratory, to ſeparate the illuminated parts from thoſe that remained dark. In doing which, ſome luminous fragments were placed upon the palm of the hand, and retained there for ſome time, when it was obſerved, that the warmth of the hand conſiderably increaſed the degree of light.

Exp. 2.

Exp. 2. Some fragments of this illuminated phofphorus were put into a fmall phial, which was then clofed with a cork, and fufpended, by a ftring, in a quart of water heated to about 120° ; by thefe means, the light was rendered much more vivid than before.

Exp. 3. Some other pieces of the illuminated phofphorus were dropped feparately into a glafs tube 32 inches long, and $\frac{7}{10}$ bore, filled with water at about 120°. The light of each piece became exceedingly bright, as foon as it entered the hot water; and they all defcended, very luminous, from the top to the bottom, fome quickly and others flowly, according to their gravity, making a very pleafing experiment.

Exp. 4. A large wooden bowl, about 12 inches wide, was next filled with water heated to about 110°, and then a quantity of illuminated phofphorus, partly in the form of powder, and partly in pieces of different magnitudes, was fcattered over the whole furface of the water; all which pieces fell, with increafed fplendour, to the bottom, where they preferved their light for fome time.

II. *The imbibed Light is extinguifhed by a great Degree of Heat.* but too much heat extinguifhes it.

Exp. 5. Some fragments of the phofphorus, rendered luminous, were expofed to a greater degree of heat, namely, by cafting them into a tin veffel containing two pints of boiling water. They flafhed with increafed light, as foon as they came in contact with the water, fell precipitately to the bottom, in a lucid ftate, and then were gradually extinguifhed.

Exp. 6. *In which the degree of heat was ftill increafed.* A fmall bar of iron, of about an inch fquare, was made red-hot, and laid horizontally in the laboratory, until, by cooling, it nearly ceafed to fhine. Some pieces of illuminated phofphorus were then put upon it in fucceffion, and the light, in a moment, glowed with uncommon luftre, but was quickly after totally extinguifhed *.

* Solar light, when received merely on a piece of white paper, may alfo be rendered more luminous by heat, and then extinguifhed by it, as appears from an experiment made by the late Mr. B. Wilfon, whofe book on phofphori I had not feen before this Paper was drawn up.

III. *The*

Latent light is excited from Canton's phosphorus by heat.

III. *The imbibed Light, after being in a latent State, is excited and rendered luminous by the Agency of Heat.*

Exp. 7. Some ſmall pieces of the phoſphorus, after having been illuminated, were depoſited in the laboratory; when the light by degrees faded away, and became totally inviſible. They were kept in this dark ſtate for the ſpace of ten days, and then placed one after another upon a heated bar of iron, as in the laſt experiment, upon which they quickly became exceedingly luminous.

From an experiment made by the ingenious Mr. Canton, I obſerve, that ſome of his phoſphorus, contained in glaſs balls hermetically ſealed, and heated in the above manner, gave a conſiderable degree of light, after it had been kept in a ſtate of darkneſs more than ſix months. Phil. Tranſ. Vol. LVIII. page 342.

§ 2.

The Effects of Cold on imbibed Light.

EXPERIMENT.

Cold extinguiſhes the ſolar light of Canton's phoſphorus, &c.

About 15 grains of the phoſphorus were put into a half-ounce phial, containing two drams of cold pump water, that had been deprived of its air by boiling. The phial was then corked, and expoſed for ſome time to ſolar light, whereby the phoſphorus became finely illuminated. In this ſtate, it was immediately put into a frigorific mixture, compoſed of ſnow and ſea ſalt, and retained there about 30 or 40 minutes, when it was taken out, and the light found to be totally extinguiſhed. The phial was then placed in ſome water, at about 60° temperature, and the light gradually revived, and became as brilliant as before it had been expoſed to the cold. This experiment was frequently repeated, and always with the ſame reſult.

I cannot but remark, that in the courſe of experiments on this ſubject, the ſuperior power of ſolar over that of ſpontaneous light was very apparent. For, the firſt trials being made in ſmall phials, containing only atmoſpheric air with the phoſphorus, the light was with ſome difficulty totally extinguiſhed; and, after the phials were taken out of the frigorific mixture,

mixture, the temperature of the laboratory would commonly foon revive the light, which rendered the experiments not altogether fatisfactory. Finding it thus fomewhat difficult to extinguifh folar light *in air*, recourfe was had *to water*, in the manner above defcribed. This anfwered perfectly well; for the water, when frozen, gave a fubftantial body, as it were, to the imbibed light of the phofphorus, fo as to enable it to retain the excefs of cold arifing from the frigorific mixture; thereby making the experiments quite fatisfactory. When the phofphorus was thus furrounded by ice, only a few minutes ftay in the frigorific mixture would generally be fufficient for a total extinction.

OBSERVATION.

From thefe experiments, compared with thofe recited in my former Paper on fpontaneous light, it appears that folar light, when imbibed by Canton's phofphorus, is fubject to the fame laws, with refpect to heat and cold, as the fpontaneous light of fifhes, rotten wood, and glow-worms.

P. S. In thefe experiments with folar light, the phofphorus was fometimes expofed to the direct rays of the fun, at other times to common day-light, in a northern afpect; and it was remarked, that it became fomewhat more luminous by mere day-light, than by the rays of the fun.

Improved preparation of Canton's phofphorus by Dr. Higgins.

It may alfo be proper to obferve, that the above experiments were made with an improved preparation of Canton's phofphorus. This improvement, which was firft made by Dr. Higgins, confifts in omitting the pulverization of the fhells. His method was, after calcining the oyfter-fhells, to put the pieces, both great and fmall, in layers, into a crucible furnifhed with a cover, and to fprinkle flowers of fulphur between each layer. After they had remained fome time in the furnace, they were taken out, fuffered to cool, and then kept in a large bottle with a glafs ftopple. For this communication, I am indebted to Mr. Lewis of Holborn, near Southampton-ftreet, who has an extraordinary dark room, where, at times, he amufes his friends with fome beautiful appearances, arifing from folar light imbibed by phofphorus prepared as above directed. A ftill further improvement of this phofphorus, it appears to me, may be made by fubftituting precipitated fulphur for the flowers of fulphur; and the experiments of this fection were chiefly made with phofphorus fo prepared.

Dark room of Mr. Lewis.

X. *Defcription*

X.

Description of a Lamp upon ARGAND'S *Principle, with Improvements, in which the Oil is maintained at the same Level by the constant Action of a Pump. By Citizens* CARCEL *and* CAREAU *.

Excellence of the lamp of Argand.

LAMPS with a double current of air are among the inventions of the eighteenth century, which are most honourable to the industry of France, and of which the general use sufficiently bespeaks the value. But it is not enough that a great light should be produced without smell or smoke, but an object of nearly equal utility consists in producing the light steadily, with economy, in the most advantageous form, and best adapted to economical purposes.

Improved by Carcel and Careau.

Citizens Carcel and Careau have succeeded in this object. We shall not dwell upon the obstacles they must have met with in arriving at the degree of perfection they have obtained; it will be sufficient to shew the value of their invention, if we can prove that it surpasses every thing of the kind which has yet been exhibited. This lamp has the double advantage of exhibiting all the good qualities of the lamp we before possessed, without their inconveniences. The nozle undergoes no alteration by heat, neither is the wick destroyed, but almost constantly preserves its whiteness.

Inconveniences of lamps of the usual construction.

The oil in ordinary lamps is liable to flow out by its expansion when heated; but in this mechanical lamp it constantly preserves its level. By dispensing with the reservoirs of oil in those lamps which are called fountain lamps, these inventors have succeeded in affording light which is not shaded on any side. In this respect the lamp is very economical, because nearly half the light of a fountain lamp must necessarily be intercepted by the receiver, which requires to be placed above the level. But in these the reservoir is in the foot, which renders them more portable; and from this circumstance, as well as the other advantages of their construction, there is no danger

* From Les Annales des Arts & Manufactures, vi. 269. I am much obliged to my correspondent C. D. for directing my attention to this article.

ger of fpilling the oil by inclining it, which is an inconvenience of great magnitude, and hitherto conftituting one of the greateft objections to the lamps in ufe.

In other lamps the flame varies in its intenfity commonly in about two hours; but in the mechanical lamp it conftantly preferves the fame fupply and the fame brilliancy.

The mechanifm adapted to the foot in order to caufe the oil to afcend, has been reduced to the greateft fimplicity; it is firm and durable, and has no communication with the oil of the refervoir. General remarks.

This lamp may be ufed in diftillations and chemical preparations, as well as in culinary purpofes; and in general we cannot do better than tranfcribe the report made to the National Inftitute by Guyton, Morveau, and Charles.

"The Clafs having charged us to examine the mechanical lamp prefented by the Citizens Carcel and Careau, at the fitting of the 21ft of laft month, as a means of adding a new degree of perfection to lamps with an inner current of air, as well as with regard to the intenfity of light as to economy and the convenience of daily ufe. Report to the Inftitute of France.

"Lamps excited by an interior current of air, of which the invention belongs to Citizen Argant, and which were announced for the firft time in February 1784*, have produced a revolution in the art of illuminating which time has ferved only to confirm, as it does all thofe which, being founded on true principles, receive the daily fanction of experience. Hiftory of the Argand lamp.

"A fhort time afterwards Citizen Lange thought of contracting the glafs chimney, fo as to direct the external current of air nearer to the flame, by which means he determined a ftill more complete combuftion of the oil, and produced a more brilliant light, without either fmoke or fmell. The union of thefe inventions feemed to have exhaufted the fubject, but Citizens Carcel and Careau apprehended that it was poffible to render the lamps ftill more perfect. They confidered that the beft lamps of this defcription do not conftantly afford the fame intenfity of light, becaufe the wick not being conftantly and alike fupplied with oil, is fubject to become charred; that it is neceffary either to raife it beyond the proper elevation, or to trim the lamp again after fome hours; and laftly, that the glafs The chimney improved by Lange. Improvements by Carcel and Careau.

* Journal de Phyfique for that month, page 159.

chimney

chimney not being capable of being made to that degree of accuracy as to afford the contraction always at the same height, the effect of this contrivance could not be the best possible, excepting when the accidental concurrence of circumstances might render it so.

" At the same time that they were busied in remedying those inconveniences, they did not neglect the means of rendering the lamp more economical, convenient, and portable; without fear of spilling the oil; without any considerable effect upon the flame by motion; without any interception of the light from a reservoir; and with the addition of an agreeable and ornamental form.

" The account resulting from our examination will enable the Class to judge of the difficulties they have had to vanquish, the ingenious processes they have used, and the success which has crowned their industry.

Experiments with the new lamp.

" The mechanical lamp was lighted before the Class for a short time. They saw the brilliant light it afforded, and compared that light with the lamps which usually illuminate their place of meeting. It continued absolutely the same five hours after it had been lighted, though the wick had not been altered or touched.

Compared with the common, it gives more than twice the light.

" In order to determine more exactly the intensity of the light, it was placed at such a distance that a body interposed between its light produced a shadow of the same obscurity as that of a common Argand's lamp. The distances were—from the mechanical lamp forty-six decimeters, and from the common lamp thirty-three decimeters, which, by the square of the distances, gives the ratio of 2116 to 1089, or 100 to 48¼, for the intensities of the light.

" The common lamps of this construction not always affording a light perfectly uniform, and the glass chimney of that which we employed not being of the most favourable dimensions, we thought it proper to endeavour to obtain a confirmation of this ratio by the comparison with candles, of which the light is less subject to vary.

Comparison with candles.

" Six candles (I suppose of wax), of the weight of one hectogram (five to the French pound), were arranged in such a manner that their flames could not mutually intercept each other. The shadow produced by the interposition of an opaque body received on a card, was found to be similar to that of the mechanical

mechanical lamp when the candles were brought to the diſtance of 566 centimeters, at the ſame time that the mechanical lamp was drawn back to 785. The ſquares of theſe numbers give the ratio of 100 to 52 nearly. If inſtead of the latter term we put the quotient of its diviſion by the number of candles, we ſee that the light of the mechanical lamp is to that of one candle as 100 to $11\frac{1}{2}$, or it would require eleven candles and a half to give the ſame light.

The heat it gives to other bodies placed over the flame.

"As this lamp may be uſed for the domeſtic purpoſes of affording heat as well as for the operations of chemiſtry even in the dry way, it becomes intereſting to aſcertain the degree of heat it was capable of communicating to veſſels placed above its glaſs chimney. For this purpoſe we took a pyrometric piece of Wedgwood not baked; it was included between two very thin capſules of platina, and placed on a ſupport, firſt at four centimeters, where, at the end of half an hour, the contraction was three degrees of the pyrometric ſcale; after which it was again placed at the diſtance of fifteen millimeters only from the upper extremity of the chimney, where, after having been kept for two hours, it was found to have paſſed the ſeventh degree, which, according to the table of correſpondence of Wedgwood, would indicate 505 degrees of the centigade thermometer.

Pyrometric piece.

Tube of glaſs.

"A tube of glaſs of five millimeters, or two lines, in diameter, of the weight of thirty-ſeven decigrams, or ſixty-ſix grains, the cube decimeter, was eaſily bended over the chimney, that is to ſay, at eight centimeters, or two inches, &c. from the upper extremity of the flame. Tin, placed in a ſmall crucible of kaolin, upon a ſupport of the ſame height, flowed in leſs than ſeven or eight minutes. Five grams, or eighty grains, of antimony in ſmall fragments, afforded in a ſimilar crucible, at the end of an hour, a degree of fuſion ſufficiently advanced to round the lower part of the button, and it is known that the loweſt eſtimation of the heat required to fuſe this metal is 431 degrees.

Conſumption of oil.

"The conſumption of oil was determined by ſeveral trials, one of which was continued for ſeven hours: It varied very little; the mean term was 34,648 grams per hour. ($1\frac{1}{4}$ oz. avoird.)

Mechanical conſtruction.

"With regard to the operative means by which theſe effects are obtained, we ſhall remark, firſt, the diſpoſition of the chimney, which can be raiſed or lowered at pleaſure by turning the cylinder

cylinder which supports it on its screw, which has nine threads; by which means the true situation to produce the best effect is obtained, whatever may be the difference of size between one glass and another. The screws are pewter, but the reporters observe, that it may be better to make one of them iron.

"The advantages of raising the oil by a pump moved by a spring are real, and prevent the wick from becoming charred for want of a constant supply of oil; besides which, as the flame is kept at a distance from the socket, it can neither calcine the metal nor deposit that crust of hardened oil which so soon alters the effects of other lamps.

The Commissaries express their approbation of the expedient by which the oil is prevented from leaking through and arriving at the mechanism, and state, that they examined the mechanism, which they find to be well calculated to produce the desired effect, and to prove as durable as could be wished.

Illumination judged by reading a book.

The brilliancy of effect is spoken of in high terms of approbation. They found that when a white gauze shade was put over the lamp, they could read in the *Anacharsis* of *Didot* at the distance of 78 decimeters, or about 24 feet; and that they could read in the same book at the distance of 89 decimeters with the flame uncovered. They do not mention the type of the book.

Blue chimney.

With a chimney of blue glass they could read at 72 decimeters.

When a ground glass was placed before the flame, they could read at the distance of 75 decimeters. A candle placed behind the same glass, gave a light by which it was difficult to read at 23 decimeters.

Particular description of the construction of the lamp.

In addition to the report of the Commission of the Institute, the Editor of Les Annales des Arts gives a more minute description, with an accurate engraving, which is presented to the reader in plate IX.

Fig. 1. View of the under part of the movement of the lamp: *aa* the lower plate of brass, which, with the superior plate connected by four brass pillars, forms the frame of the movement: *b*, ratchet wheel for winding or setting up the spring: It is pinned upon the square of the barrel arbor, and is kept in its place by the click and back spring *c*: The two holes in the face of the wheel are intended to receive the points of a key for winding it up: *d*, a plate which receives

the

the pivots: *e*, large wheel, the pinion of which is driven by the wheel of the barrel: *f f f f*, ends of the pillars: *h*, the fly and its endlefs fcrew.

Particular defcription of the conftruction of the lamp.

Fig. 2. Plan of the upper fide of the movement: *i*, upper brafs plate: *k*, the bottom of the refervoir of tin: *l l l*, connecting pillars: *m m*, a circle with its fcrews, which ferves to fupport the pump and its apparatus: *n*, body of the double force pump: *o*, aperture through which the oil rifes through the afcending pipe: The elevation of this pipe is feen at *o*, fig. 3: *p*, the double arm of the piftons of the pump: *q q*, pump rods.

Fig. 3. Lateral elevation of the mechanifm: *s s*, the plate which covers the body of the lamp, fixed by four fcrews, which at the fame time connect the lower plate *u*: This plate is raifed up in the drawing inftead of being fcrewed down, in order to give a clearer idea of the connection. The middle piece *t* is perforated by two cylindrical holes, in which the piftons move which drive the oil. The piece *t* is alfo perforated above and beneath, in order to admit of two conical valves opening upwards into each cavity or pump barrel. The lower valve in each fuffers the oil to rife into the barrel as the pifton is withdrawn, and the upper fuffers it to pafs upwards when the forcing ftroke takes place. The pieces *s*, *t*, and *u*, are fecured by a piece of leather put between them before they are fcrewed together.

The dotted line *v v* denotes part of the refervoir of oil: *w*, part of the lower plate *m*: *x*, one of the pillars: *y*, the barrel containing the fpring: *z*, pinion of the large wheel: A, pinion of the wheel of the endlefs fcrew: B, the wheel which drives a crank that works the pumps: C, the leading piece which acts on the lever: D, pinion of the wheel B. E, middle wheel, taking in the pinion D, and moved by its own pinion, which takes in the great wheel *e*: F, lever that works the arms of the pump.

Fig. 4. Elevation of the natural fize of the glafs globe *r*: 1, A pivot of fteel which paffes through the glafs globe to communicate with the arms of the pump. The lower part of this pivot is fquare, in order to receive the lever of ofcillation; the upper part is round, and has the arms of the pump driven tight upon it. 2, A brafs focket in which the pivot 1, 3 turns, and is prevented from defcending by a fhoulder. The glafs

 globe,

Particular description of the construction of the lamp.

globe, or pearl-ſhaped veſſel, is blown from a piece of barometer tube, and is connected in the ſocket by ſealing wax (or lac), which is inſoluble in oil. Citizen Carcel found a great difficulty in preventing the leakage of the oil to the works, which he has obviated by filling the glaſs veſſel *r* with molaſſes thickened by heat. This fluid is not acted on by oil, and anſwers the purpoſe very well, without impeding the motion.

Fig. 5. Elevation of a chandelier to which the mechaniſm is adjuſted. G, the foot containing the movement: H, reſervoir containing oil, and the pump: I, the ſtem of the chandelier, forming part of the reſervoir, and traverſed by the tube of aſcenſion *o*, which ſupplies the wick with oil: K, the apparatus of the lamp, having apertures through which the air enters. Fig. 6. Another more elegant figure propoſed by C. O'Reilly.

The number of the wheels are as follow: Barrel wheel, 108 teeth: It drives a pinion of 12 on the arbor of *e*, which has 84 teeth. The middle wheel E has 96 teeth, and a pinion of 12. The crank wheel has 90 teeth and a pinion of 8. The laſt or endleſs ſcrew wheel has 20 teeth, and a pinion of 12. And the ſcrew has two threads.

The movement will go twelve hours without winding up.

The uſe of the lamp is ſimple and eaſy. The wheel-work is either locked or ſet at liberty at pleaſure by a ſtop. In the regular proceſs, the wick is to be firſt trimmed; then the mechaniſm is to be wound up; and in the next place, the oil is to be poured in. When the ſtop is diſengaged, the oil is ſeen to riſe up to the wick, and this is the proper time to light it. When it is no longer wanted, the movement is to be ſtopped of courſe, at the ſame time that the lamp is extinguiſhed.

XI.

Note upon a peculiar vegetable Principle contained in Coffee. By RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A. From the Author. May 24, 1802.*

Coffee heated in water, then filtered and evaporated,

IN a veſſel calculated to confine the vapour of water, I heated a conſiderable portion of that liquid upon about a pound of raw coffee imported directly from Martinico, and of

of the quality of which I was well affured. I then filtered the liquor, and reduced it nearly to drynefs, in a glafs evaporating difh, at a gentle heat. By this means I obtained a fmall quantity of a clear yellow refiduum, like the moft tranfparent horn, and of the confiftence of honey. This refiduum did not deliquefce, or feem to be fubject to change, by expofure to the atmofphere. It was foluble in alcohol. It did not manifeft either acid or alkaline properties. By fome experiments I perceived it to be a fubftance differing effentially from all the vegetable principles with which I was acquainted; and, finding that I could obtain it pure by a method which Prouft ufed to procure tannin, I proceeded in the following manner:

gave a clear yellow refidue,

effentially different from other vegetable products.

I poured a folution of muriate of tin into fome water which had been made to boil upon coffee, and obtained a precipitate, which I collected upon a filter, and wafhed. I then put it into water, and caufed a current of fulphurated hidrogen gas to pafs flowly through the liquor. By this procefs the oxide of tin combined with the fulphurated hidrogen gas, and the fubftance originally contained in the coffee, but which, as I fhall immediately fhew, had combined with the metallic oxide, was difengaged, and remained in the liquor, while the hidrogenized fulphuret of tin was precipitated. It then remained only to evaporate the liquor to obtain the vegetable principle. In this ftate it exhibited nearly the fame appearance as before it had been combined with the oxide of tin, but feemed of a lighter colour, and more clear and tranfparent, being freed, as I fuppofe, from all extractive or other matter.

It was precipitated by muriate of tin; and the tin thrown down by fulph. hidrogen.

The difengaged new vegetable principle was thus obtained purer.

Imagining it now to be fufficiently pure, I diffolved it in a very fmall proportion of water, and examined it chemically.

Its habitudes. Soluble in alcohol, and in water: bitter tafte.

The folution was of a bright horn colour; had a bitter tafte, though not unpleafant. It was neither acid nor alkaline.

Solution of potafh, of foda, or of ammonia, poured into the liquor, changed its colour to a bright garnet red.

Alkalies turn it red;

Nitric acid produced a fimilar effect.

as did nitric acid.

Very concentrate folutions of the alkaline carbonates did not caufe a precipitate, as in a folution of tannin.

No precipitate by alkaline carbonates.

Sulphuric acid became of a dirty brown colour with the folution, but no other change was apparent.

Sulphuric acid, little change.

With muriatic, phofphoric, and the vegetable acids, there was no change but what would naturally refult from a mixture of the colours of both liquors.

Other acids, none.

Solution of iron; beautiful green; precipitate.

With any ſolution of iron in which there was not exceſs of acid, the liquor paſſed to a beautiful green; and, if it was concentrate, there was a green precipitate. Salts formed of the red oxide of iron ſucceeded the beſt; and the reciprocal action of this principle and iron is almoſt as delicate as that of either gallic acid or tannin and iron.

Muriate of tin, yellowiſh precip.

With muriate of tin there was a very abundant yellowiſh precipitate, which was a combination of the new vegetable principle with oxide of tin. Both this precipitate and that with iron are ſoluble in all the acids, and the liquors loſe their colour.

Waters of lime, barytes, ſtrontian.

Lime-water did not cauſe any precipitate in this liquor, nor did ſtrontia-water. Barytes-water gave a fawn coloured precipitate. With lime-water tannin gave a bluiſh-green precipitate; and nearly the ſame with ſtrontia-water, as alſo with barytes-water.

Gelatine gave no precipitate.

A ſolution of gelatine did not give any precipitate with this vegetable principle. The effect of tannin upon gelatine is well known.

Hence this principle is different from any other.

By theſe experiments it is proved that the principle bears ſufficient characters to diſtinguiſh it from tannin, or any other vegetable principle with which we are acquainted. The only property which it poſſeſſes in common with tannin, is its affinity for oxide of tin, while it is clearly diſtinct from tannin in every other point.

Coffee does not exhibit tannin till roaſted.

It is evident that coffee, before it is roaſted, does not contain tannin. A ſolution of gelatin poured into a decoction of well roaſted coffee, gives, however, an immediate precipitate; and the precipitate is the combination of tannin with gelatin. Meſſrs. Prouſt, Seguin, and Davy, have obſerved, that heat developes the tannin principle in many vegetables. In a commercial point of view, it might be advantageous to examine, whether thoſe vegetables are not ſuch as, before they are heated, contain this new principle. Although I did not perceive that the principle, when inſulated from the entire vegetable, was converted by heat into tannin; yet the preſence of the other component parts of the vegetable may influence the diſtribution of elements in ſuch a manner as to produce combinations different from what the ſeparate principles will afford. I have not had an opportunity of extending theſe reſearches any further in the vegetable kingdom.

Whether this new principle, *while in the vegetable*, may not be converted into tannin by heat.

XII. *Account*

XII.

Account of ſome Experiments performed upon a Scale of conſiderable Magnitude, and principally by the Agency of Froſt, to produce Sulphate of Soda, Carbonate of Magneſia, and Muriate of Ammonia, from Sulphate of Magneſia, Carbonate of Ammonia, and Muriate of Soda. By H. CAMPBELL, *M. D. From the Author.*

THE ſelected ſaline ſubſtances were, Epſom ſalt, or the ſulphate of magneſia; table ſalt, or the muriate of ſoda; and concrete hartſhorn drops, or ammoniacal carbonate. Of the proper mixtures, calculation was formed upon a ſuppoſed acquaintance with the table of agents entering into the compoſition of each body.

Sulphate of magneſia and mur. ſoda gave *ſulph. ſoda* and mur. mag. To the latter was added carbonate of ammonia, and the products were, *muriate of ammonia* and *carb. of magneſia.*

TABLE.

	Acid.	Alkaline Earth.	Water.
Sulphate of magneſia,	33 ſulphuric	19 magn.	48
Muriate of ſoda,	52 muriatic	42 —	6
Ammoniacal carbonate	45 carb. acid	43 —	12

The expected and unavoidable products were,

	Acid.	Alkali.	Water.
Glauber ſalts, or ſulphate of ſoda, containing - - - -	27 ſulph.	15 magn.	58
Salited or muriated magneſia, containing - - - -	34 mur.	41 —	25

And upon the addition of the ammoniacal carbonate to the ſalited magneſia,

Carbonate of magneſia - -	30 carbon	48 earth	22
Muriate of ammoniac - -	52 —	40 —	8

The ſalited magneſia muſt only be conſidered as an intermediate product.

To avoid the detail of various experiments, thoſe found to be moſt practicable will be only related, connected with the moſt practicable apparatus.

Detail of the experiment.

In iron pans of 150 gallons each, due proportions of ſulphate of magneſia and muriate of ſoda were put in water in proportion to the laws of ſolubility and cryſtallization. The ſaline liquor was pumped into troughs lined with tinned iron plate. The troughs were thirty feet long, one foot wide, and three inches

inches deep. Some of the liquor was expofed in butcher's trays made of afh, four feet long, two feet wide, and a few inches deep. Thermometer, Fahrenheit, 26 ——: Product, Glauber falt, or fulphate of foda, and muriate of magnefia, in quantity correfponding with the proportion of the requifites in the mixture.

Obfervations. Contrary to my ufual practice, the faline liquor in this experiment was put out in a cold ftate: The confequence was, a precipitation of the fulphate of foda in the form and appearance of fl. benzoin. The whole of the Glauber falt was thrown down; the mother liquor containing only muriate of magnefia. While the mother liquor contains Glauber falt, it will feel and appear in fome meafure harfh and faline; but when totally deprived of Glauber falt, new appearances will take place. The tinned iron gutters, inftead of fhewing a difpofition to ruft, will be remarkably clean and brilliant; the mother liquor foft and oily to the touch, and will pour fmooth. To obtain the Glauber falt in diftinct cryftals, the product of this experiment was rediffolved in a leaden evaporator, in its own water of cryftallization, and pure water added, enough to make a light faline liquor. This liquor was put into leaden coolers ten feet long, five feet wide, two inches deep, ftanding in a room, therm. 57, doors and windows were left open. In the morning I found and took about three or four large bafkets of Glauber falts, perhaps the moft perfect cryftals ever feen; cryftals from nine to fifteen inches long, and one inch broad.

Very fine cryftals of Glauber's falt.

Cryftallization by froft without fecond folution. To avoid the trouble and expence of diffolving the fulphate of foda twice, while the froft continued the operations were continued in the large way, in the proportions of fulphate magnefia, muriate foda, and water, as mentioned, with the precaution of pumping the liquor from the pans into the troughs and trays, HOT, but beneath the boiling point. The whole of the Glauber falt was always formed when the froft was fevere, as at thermom. 23, in beautiful and diftinct cryftals, half an inch broad and four or five inches long.

PRECAUTIONS AND REMARKS.

Various cautions, &c. on the management of falts. Caft iron pans, heated in frofty weather, frequently burft. Boilers fhould be made of fheet iron. Jointed wooden veffels will not hold hot faline liquors. In melting falts different fteps muft be adopted. Salt holding little water of cryftallization, as

as muriate of foda, fhould be added to a fufficient quantity of water; without a fufficient quantity of water in a heated pan it will fufe into a mafs; furface being loft, the difficulty of folution is almoft inconceivable.

Various cautions, &c. on the management of falts.

Salts holding an abundance of water of cryftallization, as fulphate of magnefia and fulphate of foda, fhould be diffolved by a gentle heat in their water of cryftallization, and what further water may be required, fhould be then added. Salts abounding in water of cryftallization, added to warm water, will conglomerate, the caloric is abforbed, and although a fmall proportion of the falt is diffolved, the remainder is made colder: it is governed by the fame law as operates on the folution of ice. The power of adding water evades a fuperabundant quantity of mother liquor, and enfures a quantity of cryftals.

To obtain the faline liquor white and pellucid, it fhould not remain long in the iron pans, not more than two hours.

The firft liquor pumped from the pans fhould be equally mixed with the laft liquor, to obtain regular cryftals in confiderable quantity. It is known that the under layers of faline liquors are heavieft.

Very light liquors afford the largeft fhoots or cryftals. Very large cryftals appear opake, and are never of a good colour; hence the neceffity of breaking down to make many furfaces, to be acted upon by light.

Heavy faline liquors, holding Glauber falts, produce a quantity of falt in a compound ftate of cryftallization.

A faline liquor, of a proper weight, will give numerous cryftals, long and narrow, and thefe will be found in fhape and colour moft perfect. Such cryftals were formed by the power of froft in the troughs and trays, acting upon *hot* faline liquor, in the *night*.

The *darkeft* nights produced the beft cryftals. Perhaps the night was colder than the day. At any rate, the maxim of the neceffity of light towards cryftallization is doubtful in this inftance. Light is not neceffary to the cryftallization of fulphate of foda.

Light not neceffary to the cryftallization of falts.

The neceffity of air towards cryftallization, as fhewn by the ufual experiment of admitting air to a folution of falt in a veffel which did not previoufly contain air, is frequently exemplified in the large way. Salts fometimes will not fhoot in a liquor

Air is neceffary.

covered

covered by a strong pellicle. The workmen break the pellicle, and the salts instantly shoot. Crystallization, under such circumstances, is often *immediately excited* by gently running a saline liquor from one gutter into another gutter.

Electricity is of consequence.

Saline liquors, under an improper atmosphere, will not form regular crystals, but accrete into shapeless lumps. The electric and non electric states of the atmosphere govern crystallization. Many a maker of Glauber salts becomes fatigued from not understanding the latter circumstance.

Effect of frost in these experiments.

From what has been said, the young chemist must not suppose that the elective attractions are altered, and the new compounds in these experiments created by means of frost. From sulphate of magnesia and muriate of soda, sulphate of soda and muriate of magnesia can be obtained without frost. Frost only possesses a readier power of converting, from what was formed in a large portion of fluid, Glauber salt or sulphate of soda, speedily, from a hot liquid into a cold fluid state. The apparent cause consists in the power of severe frost in suddenly displacing the caloric, which becomes a disaggregative power.

H. CAMPBELL.

21, *Fleet Street, May* 20, 1802.

XIII.

Note of Citizen VAUQUELIN *respecting the Boracite, called Magnesio-calcareous Borate by the French Chemists.*

Component parts of the boracite.

THIS fossil, of which the properties have afforded a number of interesting observations to philosophers and mineralogists, has been analyzed for the first time by M. Westromb, who found it to contain,

Boracic acid	68
Magnesia	13,05
Lime	11
Alumine	1
Oxide of iron	0,75
Silex	2
	95,80

Citizen

Citizen Vauquelin having examined this fubftance fome time ago with Mr. Smith, who brought a confiderable quantity, thought himfelf juftified in concluding, that the lime is no effential part in its compofition, becaufe its powder effervefces with the acids, and the fmall quantity of lime which chemifts find in their analyfis, did not appear to exceed that which the degree of effervefcence has fince indicated. They then attempted, by weak acids diluted with much water, particularly by the acetous acid, to feparate the portion of carbonate mixed with the borate; but they did not fucceed, becaufe the acetous acid, even though feebly, likewife attacked the borate. They then left the queftion undecided, for want of tranfparent cryftals which did not effervefce with the acids.

But fince that period, Mr. Stromager having fupplied Citizen Vauquelin with cryftals of this defcription, perfectly tranfparent, he fubjected them to new experiments, with the intention merely of afcertaining the prefence of lime.

Experiments which fhew that lime is not an effential part of this mineral.

He mixed their powder with muriatic acid, and when the folution was effected by means of a gentle heat, he evaporated to drynefs in order to expel the excefs of acid, and afterwards diffolved it in a fmall quantity of cold diftilled water. By this method he feparated moft of the boracic acid, which was in very white brilliant plates. He diluted the folution with water, and mixed a certain quantity of oxalate of ammonia, which, as chemifts know, is the beft re-agent to fhew the prefence of the fmalleft quantity of lime contained in any fluid, provided it contain no excefs of acid. Neverthelefs, it exhibited no fign by which the exiftence of that fubftance could be fufpected.

In order to afcertain that the fmall quantity of boracic acid diffolved by water, at the fame time as the muriate of magnefia, did not oppofe the precipitation of the lime, he mixed a portion of the muriate of lime, which did not certainly amount to a fiftieth part of the borate employed, and a cloud was immediately produced through the whole of the fluid.

On the other part, he decompofed the artificial borate of lime in the fame manner as the natural borate, and obtained, by the addition of oxalate of ammonia, a very abundant precipitate.

It is therefore evident, that if the natural borate had contained only one hundredth part of its weight of lime, it would have given fome indication by the methods employed by C. Vauquelin

quelin. He therefore concludes, that the natural magnefian borate, when perfectly tranfparent, does not contain lime, and that that which is found in opake cryftals is interpofed in the ftate of carbonate, and caufes their opacity.

This fubftance muft not therefore any longer be confidered as a triple falt, under the name of magnefio-calcareous borate, but fimply that of magnefian borate.

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XIV.

Facts and Obfervations tending to explain the curious Phenomenon of Ventriloquifm. By Mr. JOHN GOUGH *.

The power of diftinguifhing the character of found has been long confidered; but not of its direction.

IN the excellent Paper from which I have extracted the following pages, the author begins by obferving, that the power of the ear to diftinguifh very flight variations of tone has long been a fubject of univerfal notice; but that anothor very remarkable power, namely that of afcertaining the direction of found, remains ftill without explanation. We perceive not only the tone, intenfity, and character of found; but alfo whether it arrives from the right or the left, from above or below, and this with a degree of precifion which is of great and eminent utility in the concerns of life.

All founds arrive in the ear from the fame direction.

To inveftigate the foundation of this habitual judgment, we cannot have recourfe to analogy of the fenfe of hearing with that of vifion. The laft direction of the ray of light is phyfically impreffed by the pencil which enters the organ of that fenfe: but in the ear the undulations of found are all made to ftrike the inftrument of perception in the fame direction, namely that of the auditory paffage. The author, therefore, is led to attend to other facts and obfervations. He found by experiment, with an inftrument conftructed to afford the fame equal found by blows from the action of a fpring, that he could judge of an increafe or diminution of diftance, to the one hundred and twentieth part of the whole range. He alfo remarks, that

Our knowledge of the direction of found does not depend on the impulfe in the ear: but on other facts. Statement.

* Partly abridged and partly extracted from his "Inveftigation "of the Method whereby Men judge by the Ear of the pofition of "Sonorous Bodies relative to their own Perfons." Manchefter Memoirs, vol. V. part 2, page 622. London, 1802.

that found is moft effective, when its courfe is moft directly oppofed towards the organ; and fhews by experience, that the head itfelf is a fenfitive folid, capable of perceiving founds by their action upon its furface. Hence when the differences of effect of the fame found, from its difference of diftance and direction with regard to the two ears, and the two fides or portions of the head are confidered, there will be a fufficiently great variety in the fenfations to mark the circumftance of direction, and produce an habitual power of difcrimination in this refpect as in all others, which continually affect our daily operations.

Diagram to fhew the effects of found on the head.

Direct hearing.

The annexed diagram, Fig. 2, Plate VII. is given to explain the phenomena which arife from the pulfes of found being obftructed by the hearer's head, as they move in the horizontal plane paffing through his ears, which cafe ought to be treated feparately from the more complex one that comprizes the angle of elevation, along with the horizontal diftance from the axis of hearing. When the fonorous object ftands directly in front of the hearer, the femicircle A C B may be fuppofed to reprefent the horizontal fection of his head, paffing through the places of the ears, E and F, and the axis of hearing E F; alfo let G be the place of the founding body, which, according to the conditions of the cafe, lies in the plane A C B produced, and likewife in the right line G S, which bifects E F at right angles; feeing then E F is bifected by the perpendicular S G, the arch E C F is alfo bifected by the fame in the point C. Draw L G, G K, to touch the circle in T and P, then will the arcs T E C, and C F P be equal. Now all the pulfes which do not move in right lines, contained in the angles T G S and S G P fly off without touching the circle; confequently they add nothing to the found impreffed on the ears by the body placed at G, whether the places E and F be fuppofed to lie in the arcs T C and C P, or without them. But the fame number of pulfes equal in force will fall in a given time, and in fimilar directions, on the arcs T E C and C F P as well as on the ears fituated at E and F; and it is equally manifeft, that the fame reafoning will apply to two equal and fimilar folids, conftructed upon the equal and fimilar planes, E C S and F C S. Now found, though it be formed in the ears, is very much increafed by the vibrations excited in the contiguous parts of the head by the pulfes

pulses which fall upon them: therefore, as often as the two portions of the head, which are separated by the vertical plane perpendicular to the axis of hearing, are equally agitated by the pulses of the same sound, the ears are also equally affected from the same cause; which never happens, as we learn from the testimony of the other senses, unless the sounding body be placed somewhere in the right line that bisects the axis of hearing at right angles. In this manner men are taught by experience to draw a general inference from a general observation; they therefore conclude a body to be situated directly before or behind their persons, as often as the sound of it strikes both their ears with equal forces.

Phenomena of oblique hearing.

The phenomena of oblique hearing remain to be explained; which case occurs as often as the sounding body is situated in the horizontal plane, but not in the right line that bisects the axis of hearing at right angles. Let M be the place of the sounding body, and draw M O to the centre of the circle; also let O C bisect the arc E C F, and take O G in it equal to O M: also draw W M, M R, P G, G I tangents to the circle. Now suppose a sound equal to that at M, to proceed from G, then the latter would have the same effect on the arc T C P that the former has on W D R, because the arcs are manifestly equal, and alike situated relative to the points M and G. But the sound proceeding from G is a case of direct hearing, consequently the ears placed at E and F receive equal impressions from it, which is not the case with the pulses that flow from M. For though the forces imparted to the two arcs, T C P, W D R are equal, they do not fall equally on the circle in respect to the points E and F, which represent the ears; the sound therefore coming from M strikes these organs with unequal forces, as may be easily inferred from the figure.

Practical delicacy of perception as to the course of sound.

The judgment of the direction of sounds by Mr. Gough's ear was precise to about eight degrees of position as to the horizon, and about ten degrees as to elevation above its plane.

Deception of hearing from echos.

The faculty by which men judge of the direction of sounds must inevitably be liable to deception, whenever the sonoriferous undulations are made to arrive in a direction which is not from the sonorous object, as in the case of echo; and these deceptions will be the more striking, as we are apt to rely on the testimony of the sense with the most implicit confidence.

Upon

Upon this which conftitutes the fupplementary part of the prefent effay, I fhall copy my author without farther abridgement or felection, page 644.

Inftance of the fudden changes of direction of the found of bells.

"Any perfon who has had occafion to walk along a valley obftructed with buildings, at the time that a peal of bells was ringing in it, will affent to the truth of the circumftance here alluded to *. For the found of the bells inftead of arriving conftantly, at the ears of a perfon fo fituated, in its true direction, is frequently reflected in a fhort time from two or three different places. Thefe deceptions are in many cafes fo much diverfified by the fucceffive interpofitions of frefh objects, that the fteeple appears, in the hearer's judgment, to perform the part of an expert *ventriloquift* on a theatre, the extent of which is adapted to its own powers, and not to thofe of the human voice.

This effect is fimilar to that of ventriloquifm.

"The phenomenon has often attracted my attention; and the fimilarity of effect which connects it with ventriloquifm, convinces me every time I hear it, that what we know to be the caufe in one inftance is alfo the caufe in the other: I mean that the echo reaches the ear, while the original found is intercepted by *accident* in the cafe of the bells, but by *art* in the cafe of the ventriloquift. In order that the caufe which gives rife to the amufing tricks of this uncommon talent may be pointed out with the greater clearnefs, it will be proper to defcribe certain circumftances that take place in the act of fpeaking, becaufe the fkill of the ventriloquift feems to confift in a peculiar management of them. Articulation is the art of modifying the found of the larynx, by the affiftance of the cavity of the mouth, the tongue, teeth, and lips. The different vibrations, which are excited by the joint operation of the feveral organs in action, pafs along the bones and cartilages, from the parts in motion to the external teguments of the head, face, neck, and cheft; from which, a fucceffion of fimilar vibrations is imparted to the contiguous air, thereby converting the fuperior moiety of the fpeaker's body into an extenfive feat of found, contrary to general opinion, which fuppofes the paffage of the voice to be confined to the opening

The human voice does not iffue from the mouth only.

* Viz. that a fudden change of direction in found will be perceived when the original communication is intercepted, provided there be a fenfible echo.---N.

of the lips: there are but few perſons, I imagine, who have not ſome time or other witneſſed an incident, which ſhews the vulgar notion to be erroneous in this particular. For if a man ſtanding in a cloſe apartment ſhould happen to apply his face to a loop hole, or narrow window, in order to ſpeak to ſome perſon in the open air, a by-ſtander in the room with him will hear his voice, not indeed in its natural tone, but as if it were ſmothered by being forced to iſſue from a hollow caſe; but the circumſtance of his words being heard diſtinctly, by one who cannot receive them from his mouth, proves the vibrations requiſite for their production to be conveyed through the ſolid parts of the ſpeaker's body, agreeably to the preceding aſſertion. The reaſon why we generally conclude the voice to be confined to the opening of the mouth, appears to be this. Thoſe pulſes which eſcape from the aperture are the ſtrongeſt, they therefore ſurpaſs the weaker vibrations of the contiguous parts; for when a number of ſounds moving in different directions ſtrikes the ear at the ſame inſtant, the hearer does not notice their ſeveral places, but refers all of them to the quarter in which the moſt powerful is perceived. For inſtance, when a man ſtands at a ſufficient diſtance from an extenſive obſtacle, his words are anſwered by an echo; but let him make a loud uninterrupted noiſe, neither he nor any body near him hears two voices whilſt his continues, but as ſoon as the noiſe ceaſes the echo is perceived. This does not happen becauſe the one begins the moment the other ends; but the reflected ſound being the weaker of the two, it is ſmothered by that which precedes it.

Instance.

Effect of undiſtinguiſhed echos upon the voice or an inſtrument.

"We have ſeen in what manner ſecondary or reflected ſounds are ſmothered by their principals; but though the places of ſuch ſounds are not recognized by the ear, their effects do not die away unnoticed: for the reverberated pulſes mingle with thoſe which come immediately from the ſounding body, and thereby alter the ſenſation, which, without their interference, would be leſs compounded. This is the reaſon why the ſame muſical inſtrument has one tone in a cloſe chamber, where its notes undergo a multiplicity of reverberations, and another in the open air, where the reflections are few in compariſon.

Caſe of an orator;

"But it is time to apply the preceding facts to the ſubject in hand; and it will be proper to begin with a familiar example. When an orator addreſſes an audience in a lofty and ſpacious room,

room, his voice is reflected from every point of the apartment, of which all prefent are made fenfible by the confufed noife that fills up every paufe in his difcourfe; neverthelefs every one knows the true place of the fpeaker, becaufe his voice is the prevailing found at the time. But were it poffible to prevent his words from reaching any one of the audience directly, what then would follow? Undoubtedly a complete cafe of ventriloquifm would be the confequence, and the perfon fo circumftanced would tranfport the orator, in his own mind, to the place of the principal echo, which would perform the part of the prevailing found at the inftant. This he would be obliged to do, becaufe the human judgment is bound, by the dictates of experience, to regard the perfon as infeparable from the voice; and the deception in queftion would be unavoidable, being produced by the fame concurrence of caufes which makes a peal of bells, fituated in a valley, feem to change place in the opinion of a traveller. It is the bufinefs of a ventriloquift to amufe his admirers with tricks refembling the foregoing delufion; and it will be readily granted, that he has a fubtle fenfe, highly corrected by experience, to manage, on which account the judgment muft be cheated as well as the ear. This can only be accomplifhed by making the pulfes, conftituting his words, ftrike the heads of his hearers, not in the right lines that join their perfons and his. He muft therefore know how to difguife the true direction of his voice, becaufe the artifice will give him an opportunity to fubftitute almoft any echo he chufes in the place of it. But the fuperior part of the human body has been already proved to form an extenfive feat of found, from every point of which the pulfes are repelled, as if they diverged from a common centre. This is the reafon why people, who fpeak in the ufual way, cannot conceal the direction of their voices, which in reality fly off towards all points at the fame inftant. The ventriloquift therefore, by fome means or other, acquires the difficult habit of contracting the field of found within the compafs of his lips, which enables him to confine the real path of his voice to narrow limits. For he, who is mafter of the art, has nothing to do but to place his mouth obliquely to the company; and to dart his words, if I may ufe the expreffion, againft an oppofing object, whence they will be reflected immediately, fo as to ftrike the ears of the audience from an unexpected

who may become a ventriloquift.

Conditions of that art.

The found muft iffue *from the mouth only,*

and it muft be *conveyed by echo.*

unexpected quarter, in consequence of which the reflector will appear to be the ſpeaker. Nature ſeems to fix no bounds to this kind of deception, only care muſt be taken not to let the path of the direct pulſes paſs too near the head of the perſon who is to be played upon; for, if a line joining the exhibitor's mouth and the reflecting body approach one of his ears too nearly, the divergency of the pulſes will make him perceive the voice itſelf inſtead of the reverberated ſound.

Narrative of a ventriloquiſt.

"The only ventriloquiſt I ever attended, acted in ſtrict conformity to the preceding theory of this curious paradox in the ſcience of acouſtics. His audience was arranged in two oppoſite lines, correſponding to the two ſides of a long narrow room. The benches on which they were ſeated reached from one end of the place to the middle of it, the other part remaining unoccupied. The feats exhibited by him were the three following. *Firſt*: he made his voice come from behind his audience, but it never ſeemed to proceed from any part of the wall, near the heads of the people preſent; on the contrary, it was always heard reſembling the voice of a child, who ſeemed to be under the benches. He ſtood during the time of ſpeaking in a ſtooping poſture, having his mouth turned towards the place from which the ſound iſſued; ſo that the line joining his lips and the reflecting object, did not approach the ears of the company. *Second*: advancing into the vacant part of the room, and turning his back to the audience, he made a variety of noiſes, that ſeemed to proceed from an open cupboard which ſtood directly before him, at the diſtance of two or three yards. *Third*; he placed an inverted glaſs cup on the hands of his hearers, and then imitated the cries of a child confined in it. His method of doing it was this; the upper part of the hearer's arm laid cloſe along his ſide; then the part below the elbow was kept in an horizontal poſition with the hand turned downwards, which was done by the operator himſelf. After taking theſe preparatory ſteps, the man bent his body forwards in a ſituation which preſented the profile of his face nearly to the front of his hearer, whilſt his mouth pointed to the cup; in which poſture he copied the voice of a confined child ſo completely, that three poſitions of the glaſs were eaſily diſtinguiſhed by as many different tones, *viz.* when he preſſed the mouth of the cup cloſe againſt the palm, when one edge of it was elevated, and when

Voice of a child from beneath certain benches;

noiſes from a cupboard,

cries from an inverted cup.

Method of operating.

when the veffel was held near the hand but did not touch it. The fecond and third inftances of ventriloquifm afford ftrong proofs, that this delufive talent is nothing more than the art of fubftituting an echo for the primary found; for, befides the change perceivable in the direction of the voice, it was found to be blended with a variety of fecondary founds; fuch as we know by experience are produced as often as a noife of any kind iffues from a cavity. I have already made fome remarks on this fpecies of knowledge; but it would be improper to difmifs the fubject without noticing the accuracy, with which the ear recognizes the finer modifications of founds, and their caufes. I have frequently obferved, that a certain waterfall makes a flatter and duller noife when the ground is covered with fnow, than that which it affords at other feafons. The human voice alfo undergoes a fimilar change within doors, by ftriking a multiplicity of foft bodies, fuch as a number of piles of wool, or a crowded congregation in a church.

Remarks and obfervations.

The method of preventing the vibration of the vocal organs from reaching the external teguments, is ftill wanting to complete this theory of ventriloquifm; and I prefume it can only be fupplied by an adept in the art. I muft therefore difmifs the fubject unfinifhed, becaufe I have no pretenfion to that character.

Difficulty not folved.

XV.

An Analyfis of a Mineral Subftance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, *Efq.* *.

IN the courfe of the laft fummer, when I was examining and arranging fome minerals in the Britifh Mufeum, I obferved a fmall fpecimen of a dark-coloured heavy fubftance, which attracted my attention, on account of fome refemblance which it had with the Siberian chromate of iron, on which at that time I was making experiments.

Specimen of mineral obferved in the Britifh Mufeum.

Upon referring to Sir Hans Sloane's catalogue, I found that this fpecimen was only defcribed as " a very heavy black ftone,

Hiftorical particulars.

* Philof. Tranf. 1802.

"with golden ſtreaks," which proved to be yellow mica; and it appeared, that it had been ſent, with various ſpecimens of iron ores, to Sir Hans Sloane, by Mr. Winthrop, of Maſſachuſets. The name of the mine, or place where it was found, is alſo noted in the catalogue; the writing however is ſcarcely legible: it appears to be an Indian name, (Nautneauge) but I am informed by ſeveral American gentlemen, that many of the Indian names (by which certain ſmall diſtricts, hills, &c. were forty or fifty years ago diſtinguiſhed,) are now totally forgotten, and European names have been adopted in the room of them. This may have been the caſe in the preſent inſtance; but, as the other ſpecimens ſent by Mr. Winthrop were from the mines of Maſſachuſets, there is every reaſon to believe that the mineral ſubſtance in queſtion came from one of them, although it may not now be eaſy to identify the particular mine.

§ I. DESCRIPTION OF THE ORE.

Deſcription.

The external colour is dark browniſh gray.

The internal colour is the ſame, inclining to iron gray.

The longitudinal fracture is imperfectly lamellated; and the croſs fracture ſhews a fine grain.

The luſtre is vitreous, ſlightly inclining in ſome parts to metallic luſtre.

It is moderately hard, and is very brittle.

The colour of the ſtreak or powder is dark chocolate brown.

The particles are not attracted by the magnet.

The ſpecific gravity, at temp. 65°, is 5918 *.

Experiment 1.

The ore was digeſted in muriatic acid.

Some of the ore, reduced to fine powder, was digeſted in boiling muriatic acid for about one hour.

Little action.

* The following reſults of ſome experiments which I have purpoſely made, will ſhew how much the ſpecific gravity of this ore is different from that of Wolfram, and Siberian chromate of iron.

Pure Wolfram, free from extraneous ſubſtances, at tem. 65°	6955.
Siberian chromate of iron, containing ſome of the green oxide	3728.
Pure Siberian chromate of iron - - -	4355.

The Siberian chromate of iron, like all other mineral ſubſtances which are not cryſtallized, and which conſequently are not always homogeneous, muſt evidently be liable to conſiderable variations in ſpecific gravity.

The

The acid appeared to have acted but flightly upon the powder; as the former remained colourlefs, and the latter did not feem to be diminifhed. A portion, however, chiefly of iron, was found to be diffolved; for ammonia formed a yellow flocculent precipitate; pruffiate of potafh produced one which was blue; and tincture of galls, when the excefs of acid had been previoufly faturated by an alkali, formed a precipitate of a rich purplifh brown colour.

Experiment 2.

In nitric acid: little action.

Another portion of the powder was, in like manner, digefted with nitric acid; but, excepting fome flight traces of iron, this acid afforded nothing worthy of notice; the action of it upon the ore, was indeed fcarcely perceptible.

Experiment 3.

In fulphuric acid. Slight action.

Some of the pulverized ore was digefted with concentrated fulphuric acid, in a ftrongly-heated fand-bath, until nearly the whole of the acid was evaporated; the edges of the mafs then appeared blueifh, and became white, when boiling diftilled water was added.

This acid certainly acted much more powerfully than thofe which have been mentioned; but ftill only a fmall part of the ore was diffolved. It muft however be obferved, that a very copious blue precipitate was obtained by pruffiate of potafh; a plentiful purplifh brown precipitate was alfo produced by tincture of galls, after the excefs of acid had been faturated by an alkali; and, laftly, when the yellow ferruginous precipitate formed by ammonia was diffolved in diluted nitric acid, fome white flocculi remained, which were completely infoluble in the acid, even when it was added fo as to be in confiderable excefs.

From thefe experiments it was evident, that the ore could not readily be decompofed by the direct application of the mineral acids; and I therefore had recourfe to the following method, which has frequently been employed with fuccefs in fimilar cafes.

ANALYSIS.

A.

Fufion with carbonate of potafh.

A mixture of 200 grains of the powdered ore with five times the weight of carbonate of potafh, was expofed to a ftrong red

 heat,

heat, in a filver crucible. As foon as the matter began to flow, a very perceptible effervefcence took place; and, when this had fubfided, the whole was poured into a proper veffel.

The mafs, when cold, was grayifh-brown.

Solution in water left a brown refidue.

Boiling diftilled water was poured upon it; and the brown refiduum, which was confiderable, was well edulcorated upon a filter.

Precip. by nitrous acid: white flakes.

The filtrated liquor had a flight yellowifh tinge, and, being fuperfaturated with nitric acid, afforded a copious white flocculent precipitate, which fpeedily fubfided; but, although a very confiderable additional quantity of nitric acid was poured upon the precipitate, it was not re-diffolved.

The refidue did not yield to fufion by potafh.

The refiduum of the ore was dark brown, and was again melted with potafh. and treated as before; but fcarcely any effect was thus produced; the alkali was therefore wafhed off, and the powder was digefted with muriatic acid, which foon affumed the deep yellow colour ufually communicated to it by iron. After half an hour, the acid was decanted, and the refiduum was wafhed with diftilled water.

It was therefore digefted with mur. acid, which took up iron.

This refidue was fufed with potafh, and precipitated as before.

This powder was now of a much paler colour; and, being mixed with potafh, it was melted and treated as before. A confiderable precipitate was again obtained by the addition of nitric acid; and the refiduum, after being digefted with muriatic acid, was again fufed with potafh, by which means the whole was completely decompofed, after about five repetitions of each operation.

What was left was treated in the fame manner.

B.

The muriatic folution faturated with ammonia afforded *the iron.*

The muriatic folution was diluted, and, being faturated with ammonia, afforded a plentiful ochraceous precipitate; which again was diffolved in cold dilute nitric acid, and afforded a fmall quantity of a white infoluble fubftance, fimilar to that which was obtained from the alkaline folution. From this nitric folution, I then obtained, by means of ammonia, a precipitate of oxide of iron, which, being properly dried, weighed 40 grains.

C.

The nitric precipitates were a *white matter.*

The different alkaline folutions which had been made fubfequent to that which has been firft mentioned, were mixed together, and, being fuperfaturated with nitric acid, afforded the fame white infoluble precipitate; the total quantity of which, obtained from 200 grains of the ore, amounted to about 155 grains.

The

The liquor from which this precipitate had been feparated by nitric acid, was then faturated with ammonia, and, being boiled, afforded about two grains of oxide of iron. and the fluid gave a little more *iron*.

I obtained, therefore, from 200 grains of the ore,

	Grains.	Grains.
Oxide of iron - - -	42	= 197.
And of the white precipitated fubftance	155	

But, as I could not repeat the analyfis without deftroying the remaining part of the only fpecimen at prefent known of this ore, I do not wifh the above ftated proportions to be regarded as rigidly exact; it will be fufficient, therefore, to fay at prefent, that the ore is compofed of about three parts of the white matter, and rather lefs than one of iron.

§ II. PROPERTIES OF THE WHITE PRECIPITATE.

A.

It is of a pure white, and is not extremely heavy. The white precipitate

It has fcarcely any perceptible flavour, nor does it appear to be foluble in boiling water; when, however, fome of the powder is placed upon litmus paper moiftened with diftilled water, the paper in a few minutes evidently becomes red.

B.

1. When examined by the blow-pipe, it is not fufible *per fe* in a fpoon of platina, nor upon charcoal, but only becomes of a lefs brilliant white. is infufible,

2. Borax does not appear to act upon it; for the white particles are only difperfed throughout the globule. not affected by borax.

3. It produces an effervefcence when fufed with carbonate of foda, and forms a colourlefs falt; but, if too much of it be added, then the mafs, when cold, appears like a white opaque enamel. Soluble by fufion with foda,

4. When carbonate of potafh is employed, the effects are fimilar in every refpect to thofe of foda; and it may here be remarked, that the faline combinations thus formed with foda, or potafh, are foluble in water; and that thefe folutions have the fame properties as that which was formed when the ore was decompofed by an alkali. The portion of the white precipitate which may be in excefs, fubfides unaltered, when the globules are diffolved in water. and with potafh, as in the firft decompofition.

Phoſphate of ammonia forms a deep blue globule.

5. Phoſphate of ammonia produces a very marked effect; for, when melted in a platina ſpoon, if ſome of the white ſubſtance be added, a conſiderable effervefcence takes place, and the two ſubſtances rapidly unite. The globule, when cold, is deep blue, with a tinge of purple, but, when held between the eye and the light, it appears of a greeniſh gray colour.

C.

It reſiſts nitric acid:

It is perfectly inſoluble, and remains unchanged in colour, and in every other reſpect, when digeſted in boiling concentrated nitric acid.

D.

is ſoluble in ſulphuric acid: which by dilution lets fall a ſulphate.

It is diſſolved by boiling ſulphuric acid, and forms a tranſparent colourleſs ſolution, which is however only permanent while the acid remains in a concentrated ſtate; for, if a large quantity of water be added to the ſolution, or if the latter be poured into a veſſel of diſtilled water, the whole in a few minutes aſſumes a milky appearance, and a white precipitate is gradually depoſited, which cracks as it becomes dry upon the filter, and, from white, changes to a lavender blue colour, and again, when completely dry, to a browniſh gray. It is then inſoluble in water, has not any flavour, is ſemi-tranſparent, and breaks with a gloſſy vitreous fracture.

This ſubſtance is much heavier than the original white precipitate; and in a very ſlight degree may be diſſolved by boiling muriatic acid, or by boiling lixivium of potaſh.

Upon examining theſe ſolutions, I found that both contained the original white ſubſtance, together with ſome ſulphuric acid; ſo that the precipitate obtained from the ſulphuric ſolution by the addition of water, is a ſulphate of the white matter *.

The whole is not however precipitated by water; for a part remains in ſolution, which may be ſeparated from the ſulphuric acid by either of the fixed alkalis, or by ammonia.

The ſulphuric ſolution afforded an olive precipitate by pruſſiate of potaſh;

The ſulphuric ſolution is not rendered turbid by the addition of water, until ſome minutes at leaſt have elapſed; when, therefore,

* This ſulphate is alſo precipitated when the ſulphuric ſolution has been long expoſed in an open veſſel to the air; and, according as this may be moiſt or dry, the effect is produced ſooner or later.

therefore, fome pruffiate of potafh was added immediately after the water, the colour of the liquor became olive green, and a copious precipitate, of a beautiful olive colour, was gradually depofited.

Tincture of galls, after a few minutes, caufed the liquor to become turbid, and a very high orange-coloured precipitate was obtained. orange by tinct. galls,

A few drops of phofphoric acid were added to a part of the concentrated fulphuric folution; and, after about 12 hours, the whole became a white opaque ftiff jelly, which was infoluble in water. jelly by phofphoric acid,

Potafh, foda, and ammonia, whether pure or in the ftate of carbonates, feparate the fubftance in queftion from the fulphuric folution, in the form of a white flocculent precipitate; and, when thefe alkalis are added to a confiderable excefs, they do not rediffolve the precipitate, unlefs they are heated; then, indeed, the fixed alkalis act upon it, and form combinations which have already been mentioned, but which we fhall foon have occafion more particularly to notice. white flakes by alkali, alkaline combinations.

E.

1. The white precipitate, when recently feparated from potafh, is foluble in boiling muriatic acid; and this folution may be confiderably diluted with water, without any change being produced. Muriatic folution of the white precip.

2. A part was evaporated to drynefs, and left a pale yellow fubftance, which was not foluble in water, and was diffolved with great difficulty, when it was again digefted with muriatic acid. Evap. left a yellow refidue.

3. Pruffiate of potafh changed the colour of the muriatic folution to an olive-green; the liquor then gradually became turbid, and an olive-coloured precipitate was obtained, fimilar to that which has been lately mentioned. But, olive precip. by pruffiate.

4. If fome nitric acid was previoufly added to the muriatic folution, then the pruffiate changed the liquor to a grafs-green, but did not produce any precipitate. but none if nitric acid firft added.

5. Tincture of galls, in a few minutes, formed an orange-coloured precipitate, like that which has been mentioned; but, if the acid was in too great an excefs, it was neceffary to add a fmall quantity of lixivium of potafh or foda, before the precipitate could be obtained. Orange pr. by galls.

6. A

White flakes by phos. acid,

6. A small quantity of phosphoric acid, being added to the muriatic solution, in a few hours formed a white flocculent precipitate.

and by alkalis.

7. Potash, soda, and ammonia, also produced white flocculent precipitates, which were not redissolved by an excess of the alkalis, unless the liquors were heated; and, in that case, part was dissolved by the fixed alkalis, but not by ammonia.

Muriates of lime, magnesia, and strontian, gave no precip.

8. The muriatic solution did not yield any precipitate, when the muriates of lime, magnesia, and strontian, were added; but muriate of barytes formed a slight cloud.

Zinc threw down white flakes.

9. When a piece of zinc was immersed in the muriatic solution, a white flocculent precipitate was obtained *.

F.

Acetous acid does not dissolve the white matter.

The acetous acid has not any apparent effect on the white precipitate, when long digested with it.

G.

Fixed alkalis readily combine with it.

The fixed alkalis readily combine with this substance, both in the dry and in the humid way.

We have already seen, that the former method was employed with success in the analysis of the ore; and the experiments made with the blow-pipe may be regarded as an additional confirmation. In each of these cases, the white precipitate combined with the alkali, as soon as the heat was sufficient to cause the latter to flow; and, when a carbonate was employed, a portion of carbonic acid was expelled.

The carbonic acid was in like manner disengaged, when the white precipitate was boiled with lixivium of carbonate of potash, or of soda; and the solutions thus prepared, resembled in every respect those which were formed by dissolving in water the salts which had been produced in the dry way.

It will be proper here to give a more particular account of these combinations.

Humid solution in potash: a portion left,

1. Some of the white precipitate was digested, during nearly one hour, with boiling lixivium of pure or caustic potash:

* This appears to indicate the obstinacy with which this substance retains a certain portion of oxigen; for we here see that zinc does not precipitate it in the metallic state, but only reduces it to an insoluble oxide.

about

about one-fourth of the powder was diffolved; and the remainder, which appeared little if at all altered, fubfided to the bottom of the veffel.

The clear folution, which contained a great excefs of alkali, was decanted; and, by gentle evaporation, yielded a white glittering falt, in fcales, very much refembling the concrete boracic acid. affords a fcaly glittering falt;

The falt was placed upon a filter, fo that the lixivium might be feparated. It was then wafhed with a fmall quantity of cold diftilled water; and, being dried, remained as above defcribed, although conftantly expofed to the open air. permanent in the air;

This falt had an acrid difagreeable flavour, and contained a fmall excefs of alkali. It did not diffolve very readily in cold water; but, when diffolved, the folution was perfect and permanent. acrid tafte: foluble in water;

Some nitric acid was added to part of the folution, and immediately rendered it white and turbid. In a fhort time, a white precipitate was collected, fimilar to that which had been employed to neutralife the potafh: and the clear fupernatant liquor, being evaporated, only afforded nitre. precipitable by nitric acid.

Pruffiate of potafh was added to another portion; but did not produce any effect, until fome muriatic acid was dropped into the liquor, which then immediately affumed a tinge of olive green, and flowly depofited a precipitate of the fame colour. Pruffiate of potafh gave no precipitate unlefs acid was added;

Tincture of galls did not affect the folution at firft; but, when a few drops of muriatic acid had been added, it gradually loft its tranfparency, and yielded an orange-coloured precipitate. nor did galls.

2. As fo large a part of the white precipitate had remained undiffolved in the foregoing experiment, it was digefted again with another portion of the fame lixivium, but without any effect. I therefore wafhed off the alkali, and boiled fome nitric acid with the powder, until the acid was completely evaporated. After this, the powder was expofed to a ftrong heat in a fand-bath. It was then again digefted with the lixivium, and a part was diffolved as before; but ftill the refiduum required to be treated with nitric acid, before the alkaline liquor could again act upon it; fo that it was neceffary to repeat thefe alternate operations feveral times, before the whole of the powder could be united with the alkali. The portion not diffolved in potafh—was not foluble till after treatment with nitric acid. It left a refidue having the fame properties.

3. When

Carbonate of foda, or potafh, acted very nearly as pure potafh.

3. When the white precipitate was digefted with folution of carbonate of potafh, or of foda, it was diffolved, much in the fame manner as above related; and the properties of the folutions, when examined by reagents, were alfo fimilar, excepting that the orange-coloured precipitates produced by tincture of galls were of a paler colour.

Precip. by the tungftate and molybdate of potafh, and cobaltate of am.

Tungftate of potafh, molybdate of potafh, and cobaltate of ammonia, being feverally added to the folution of the white fubftance in potafh, produced white flocculent precipitates.

Hydro-fulph. am. gave redifh precip.

Hydro-fulphuret of ammonia produced a reddifh chocolate-coloured precipitate.

The white matter diffolved in alkali, and iron diffolved in alkali, gave, by mixture, a precipitate *of the original ore.*

4. As the ore was decompofed by being fufed with potafh, the following experiment affords a curious inftance (among the many already known) of the change in the order of affinities produced by a difference of temperature.

Some of the folution of the white precipitate in potafh, was poured into the alkaline folution of iron, which was formerly known by the name of Stahl's *Tinctura Alkalina Martis.* Potafh was in excefs in both of thefe folutions; but neverthelefs a cloud was immediately produced, and a brown ferruginous precipitate was depofited.

Part of this precipitate was diffolved in muriatic acid; and the folution, being examined in the ufual way, yielded a blue precipitate when pruffiate of potafh was added, and a purplifh brown precipitate with tincture of galls.

The other part of the precipitate was digefted with dilute nitric acid; which diffolved the ferruginous part, but left untouched a white flocculent matter, perfectly refembling the fubftance which has been fo often mentioned. The precipitate therefore produced by the mixture of the two alkaline folutions, was a combination of the white matter with oxide of iron, very fimilar to the original ore.

(To be continued.)

XVI.

Remarks on the Mamoth. By Louis Valentine, *Phyfician in Chief of the Army and Hofpitals of America, of feveral National and Foreign Societies, refident in Nancy.*

Bones of a large extinct animal found in America.

SINCE fettlements have increafed in North America, a great quantity of bones have been found belonging to fome extinct animal, which feems to have refembled an elephant, but

was

was much larger. By digging near the lakes of Canada, where the animal is called by the savages, the Father of Oxen; near the rivers which fall into the Ohio; towards the rivers Miami, Muskingum, in the state of Kentucky, and of Tennessee, &c. &c. but principally near the salt springs, pieces of skeletons and tusks have been found, of an astonishing length and weight.

We have seen a femur and a tibia, which, when united, must have been five feet and a half high; another femur, which was alone five feet long, and thirty-six inches in circumference in its middle or cylindrical part; ivory tusks resembling those of an elephant, which were near seven feet long, and one foot six or eight inches in circumference at the base. It was not till the year 1800 that a complete skeleton of these fossil bones could be procured. Two physicians of Philadelphia, Doctors Barton and Wistar, had in their possession the lower jaw almost entire, with two teeth on either side, in particular, that of the former has five and three points, all quite double; but no one had the entire head. **Enormous magnitude by admeasurement.**

The state of New York (in the environs of the beautiful river Hudson) has of late years been the theatre of discoveries of fossil bones, a greater quantity of them having been found there than any where else. In 1800, by digging in the low and marshy places of the counties of Orange and Ulster, at three, four, and five feet deep, parts, which had never been before discovered, were found. Some bones, ten feet deep in the earth, were as sound and entire as those which had been met with nearer the surface. Some, however, were found broken, particularly those of the head. **Where most plentifully found.**

In another place, eight miles from the city of New York, an upper jaw was found perforated to receive a tusk like that of an elephant; the connection of the tusks was by *gomphosis;* the tusks were evidently of ivory; the openings for the nostrils were eight inches in diameter; and notwithstanding that the bones of the feet afford reason to conclude that the animal had claws, it is scarcely possible to avoid thinking, from the structure of the head, that it was a species of elephant. Some hair has even been found, three inches in length and of a dark colour, which is said to have belonged to this monstrous quadruped. Doctor Graham, a Senator, in a letter to Doctor Mitchill, Representative in Congress, and Professor of chemistry **Particular facts.** **Conjectures as to the species of animal.**

and

and natural hiftory at the college of Columbia, fays on this occafion, that it is difficult to refolve the queftion; "Why Providence fhould have deftroyed this fpecies, which it was pleafed to create?" Yet if it was voracious, it is happy for the human fpecies that it has by any means become extinct.

Queftion as to final caufes.

The animal has never been feen within human memory or tradition.

We may reafonably doubt the affertion of thofe authors who affirm, that certain favages, Ruffians and Greenlanders, have feen this animal living, and that it ftill exifts in the north. All that has been tranfmitted by tradition to the oldeft Indians who have communication with the United States, and from whom information has been fought concerning this object, is quite fabulous, and does not offer even a fhadow of probability. Mr. Jefferfon, now Prefident of the United States, formerly paid great attention to this fubject. (See his Account of Virginia.)

Conjectural remarks.

It is certain that in Siberia and in Greenland, where fimilar foffil bones have been found, no one has brought proof of having feen this animal living. We cannot fuppofe it to be cetaceous or amphibious, of the nature of the hippopotamus, as there are fufficient reafons to prove the contrary. Mr. Cuvier, whofe refearches into the extinct fpecies of animals are no lefs curious than fcientific, diftinguifhes the animal of Siberia, which affords foffil ivory, from the mamoth, which latter differs principally from the former on account of its magnitude, and the points of its grinders, &c.

An entire fkeleton,

Since the time of our having received details on this fubject (fee the New York Medical Repofitory, Vol. IV.) and what we have related elfewhere (fee the third edition of Guthrie's Geography, in French, Vol. VI. page 225 and 262, publifhed by Langlois at Paris), an account has been publifhed in the American papers in 1801, that Mr. William Peal, proprietor of the mufeum at Philadelphia, having collected the bones found in the county of Orange, in the ftate of New York, had fucceeded in forming a fkeleton, the height of which is twelve feet; the head being four feet and a half in length, and the tufks ten feet; the other parts being in the fame proportion.

twelve feet high, and its head four feet and a half long.

Local fituation of thefe enormous bones.

Almoft all thefe bones have been found in calcareous earth. The bones of the megalonix, or great claw, of which Mr. Jefferfon has given a defcription, were found in caverns of lime ftone and chalk in Tennefee. The other enormous bones of the megatherium, found in fuch great quantities in the county of Ulfter, of which Sylvanus Miller, Efq. has given fome

detail

detail in a letter to Profeffor Mitchill, are found in ftrata of marl, which are dug to procure this calcareous fubftance as a manure for land. It is, neverthelefs, fometimes remarked, that thefe bones begin to fall in fmall portions when, after having been from their calcareous inclofure, they are expofed to the atmofphere. Teeth, which were found and entire when extracted from the earth, become black in a fhort time, crack, lofe their enamel, and fall into fmall fcales. Without fuch a prefervative we may prefume, with our friend Mitchill, that the remains of thefe animals would have been decompofed many centuries ago.

They fometimes perifh by expofure to the air.

SCIENTIFIC NEWS, &c.

Prizes of the National Inftitute of France.

THE clafs of mathematical and phyfical fciences having propofed in the year 8, as the fubject of a prize to be awarded at the public fitting of the 15 Germinal, in the year 10, the following queftion: *What are the characters which diftinguifh in vegetable and animal matters, thofe which ferve as ferments, from fuch other bodies as they put into a ftate of fermentation?* And the memoirs which have been received not anfwering the conditions of the program, the clafs propofes the fame fubject again for the year 12. The prize is of the value of one kilogramme (about one hundred and twenty-five pounds fterling). It will be given at the public meeting of the 15 Germinal, in the year 12. Memoirs muft be fent before the 1ft Nivofe of the fame year.

Prize medal for a memoir on ferments and fermentable bodies

Aftronomical Prize.—Citizen Lalande has prefented the National Inftitute of France with the fum of ten thoufand francs (about 400 guineas) to found an annual prize, to be given by the Inftitute to the author of fuch difcovery, obfervation, or work in aftronomy, as fhall be thought the moft remarkable or ufeful, during the courfe of the year. The Inftitute very highly applauded this act of generofity in one of its members, and decreed, that thanks fhould be configned in their regifters, and commiffaries nominated by each of the three claffes, to prefent at the next general meeting the means of execution.

Aftronomical prize.

Communications to the Royal Society refpecting the Planet Ceres.

Dr. Herfchel fent an account of the appearance of the new planet, as viewed through his telefcopes. He had fought for it

Extreme fmallnefs of the planet Ceres.

it in vain, until he received Dr. Maſkelyne's determination of its place. When viewed with powers of 600 and 1200, it could not be decidedly diſtinguiſhed from a ſtar, until it was found to change its place. Its apparent diameter was not large enough to be directly determined, but it was certainly not larger than one-fourth of that of the Georgian planet, and perhaps equal only to one-ſixth. From a rough computation of its magnitude, Dr. Herſchel concludes that its real diameter is about $\frac{4}{5}$ of that of the moon: its light is of a reddiſh hue.

Mr. Gilpin alſo gave the Society an account of obſervations on the 8th and 12th of February. He found the planet's right aſcenſion change from 188° 41′ to 188° 30′, while its declination increaſed. Mr. Gilpin obſerves that its light reſembles that of the planet Mars.

Nebulous atmoſphere. Thurſday, 25th February. A letter from Mr. Schroeter of Lilienthal, reſpecting the planet Ceres Ferdinandia, informed the Society that Mr. Schroeter had obſerved a nebuloſity round the planet, ſomewhat reſembling that of a comet: the diameter of the true diſc being 1.8″, and that of the nebula 2.6″, but the diſtinction was not always equally obſervable. Mr. Schroeter conſiders this body as of a hybrid nature, or a medium between a planet and a comet; but he imagines the apparent nebuloſity to be owing to an atmoſphere, and that, according to the different ſtates of this atmoſphere, the light reflected from the planet is either white, bluiſh, or reddiſh.

A table of obſervations of the ſame planet was alſo communicated by Mr. Mechain, through Sir Henry Englefield.—*(This article is taken from the Journal of the Royal Inſtitution.)*

Leonardo da Vinci.

The lovers of the polite arts will be pleaſed to learn, a new tranſlation of Leonardo da Vinci's treatiſe on painting (for which Pouſſin made the figures) will ſoon be ready for publication. This work has been long in the hands of Mr. Rigaud, R. A. who has paid it particular attention and care; and has given new importance and energy to the work, by arranging the chapters ſucceſſively under proper heads; by which the ſtudent will be much facilitated in underſtanding the precepts of this great maſter in the art of painting.

The reader will recollect ſome particulars of the extraordinary reſearches and very ſuperior genius of Da Vinci, by the extracts given in the former ſeries of this Journal, from an abridgement of his writings, by J. B. Venturi, quarto, II. 84.

Experiments

Experiments to prove that all Bodies, whatever may be their Nature, are obedient to the Action of Magnetism, and that this Action is sufficiently powerful to admit of being measured. By Citizen COULOMB *.

It has long ago been remarked, that platina, nickel, and several other bodies, acquire a sensible degree of magnetism; but some philosophers attribute this property only to a portion of iron not easy to be separated, and conclude, that by obtaining a greater degree of purity, we might succeed in rendering them perfectly indifferent to the action of the magnetic bar.

Magnetism of nickel, &c. supposed to be accidental.

The new experiments which Citizen Coulomb has made and repeated before the Institute, lead us on the contrary to think, that the action of magnetism extends through all nature; for none of the bodies he has yet tried was found to resist this power.

All bodies are magnetical.

But however real this action may be, it is not alike in all bodies, and in most of them it must be necessarily very small, to have escaped the attention of philosophers to this time. In order, therefore, to exhibit and to measure these results, we must begin by placing the bodies in a situation which shall allow them to yield to the weakest action.

For this purpose, Citizen Coulomb fashioned his subjects into the form of a cylinder or small bar, and in this state he suspended them to a silken thread, such as is drawn from the silk worms' cone, and in this state he placed them between the opposite poles of two magnetic bars of steel. The single thread of silk could hardly bear the weight of a quarter of an ounce without breaking, consequently it became necessary to form small bars very light and thin. Citizen Coulomb made them about seven or eight millemetres in length (or less than half an inch), with three quarters of a millemetre (or about an hundredth part of an inch) in thickness, and he gave the metals about one-third of this thickness.

Experiments made with bodies suspended to a single fibre of silk.

In his experiments he placed the steel bars in the same right line, their opposite poles being five or six millemetres farther asunder than the length of the needle intended to oscillate between them. The result of the experiment shewed, that whatever might be the substance of the needle, it always disposed

All bodies assumed the direction of two magnets which were applied.

* Communicated to the French National Institute, and inserted in the Magazin Encyclopedique, No. 22, an 10.

itſelf according to the direction of the two bars; and that if they were turned from this direction, they always recovered it, after oſcillations of which the number was often more than thirty per minute. It was therefore eaſy in every caſe to determine, from the weight and figure of the needle, the force which had produced the oſcillation.

Subjects of experiment.

Theſe experiments were ſucceſſively made with ſmall needles of gold, ſilver, copper, lead tin, ſmall cylinders of glaſs, a piece of chalk, a fragment of bone, and different kinds of wood.

Extreme delicacy of the ſuſpenſion.

Citizen Coulomb has proved, in a former memoir, that the force of torſion of the ſilk thread is ſo ſlight, that in order to draw it round through the entire circle, it would require a force ſcarcely equal to the one hundred thouſandth part of a gram (or about one ſeven hundredth of a grain). A quantity ſo minute cannot therefore ſenſibly derange the meaſure of magnetic force in the different bodies, and its effect even, if it were admitted to be of perceptible magnitude, may alſo be urged in proof of the general concluſion of Citizen Coulomb; becauſe the magnetic power muſt overcome this reſiſtance of the thread in order to manifeſt itſelf. Our author gives, in the third vol. of the Memoirs of Natural Philoſophy and Mathematics of the National Inſtitute, a very ſimple formula to determine the magnetic force of a body from the time of its oſcilations, and he means to ſhew in another memoir, the method of determining this reſult in different bodies of the ſame figure placed between the poles of two bars. He thinks it now proved, that all the elements which enter into the compoſition of our globe, are ſubjected to the magnetic power, and that the whole maſs collectively forms one ſingle magnet.

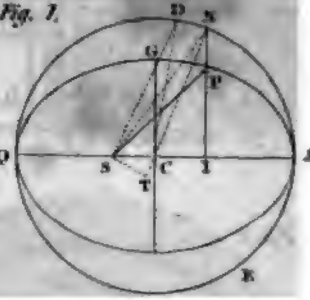
Obſervations.

In favour of thoſe who might be deſirous of repeating his experiments, and rendering them very ſenſible, the author remarks, that the method of ſucceeding conſiſts in diminiſhing the ſize of the oſcilating bodies. From ſome eſſays, of which the reſults terminate this memoir, it ſeems to follow, that the accelerating forces are inverſely as the maſſes are very nearly in the direct proportion of the ſurfaces; but Citizen Coulomb gives this rule only as a firſt deduction, which requires to be confirmed.

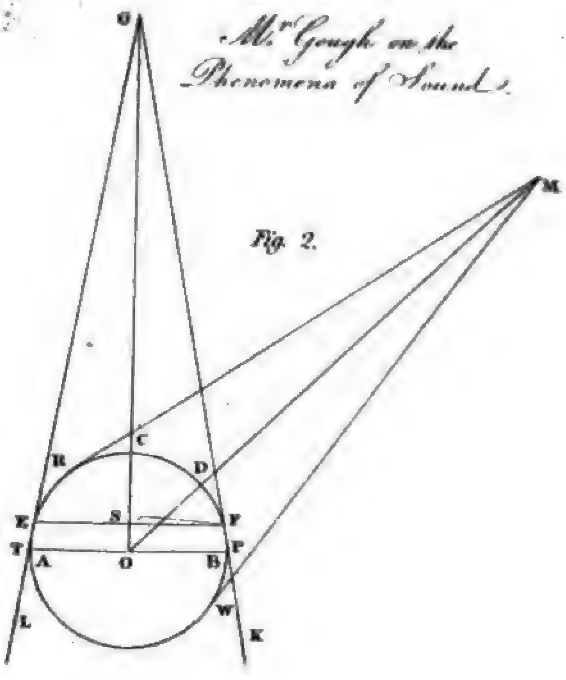
ERRATA.

In Mr. Cruickſhank's paper, p. 43, end of the firſt paragraph, for *faults* read *facts*; and p. 46, l. 26, for *computed* read *compelled*.

Mr. Gregory's Investigation of Mr. Pearson's Analogy, &c.

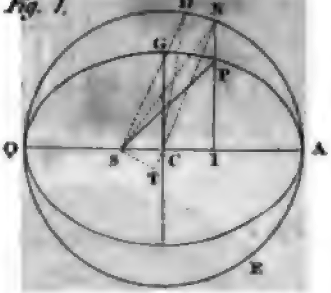


Mr. Gough on the Phenomena of Sound.



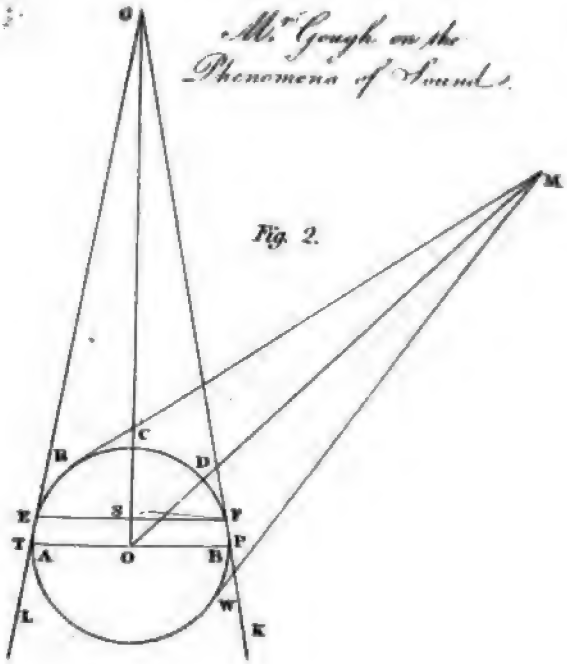
Mr. Gregory's Investigation of Mr. Pearson's Analogy &c.

Fig. 1.



Mr. Gough on the Phenomena of Sound.

Fig. 2.



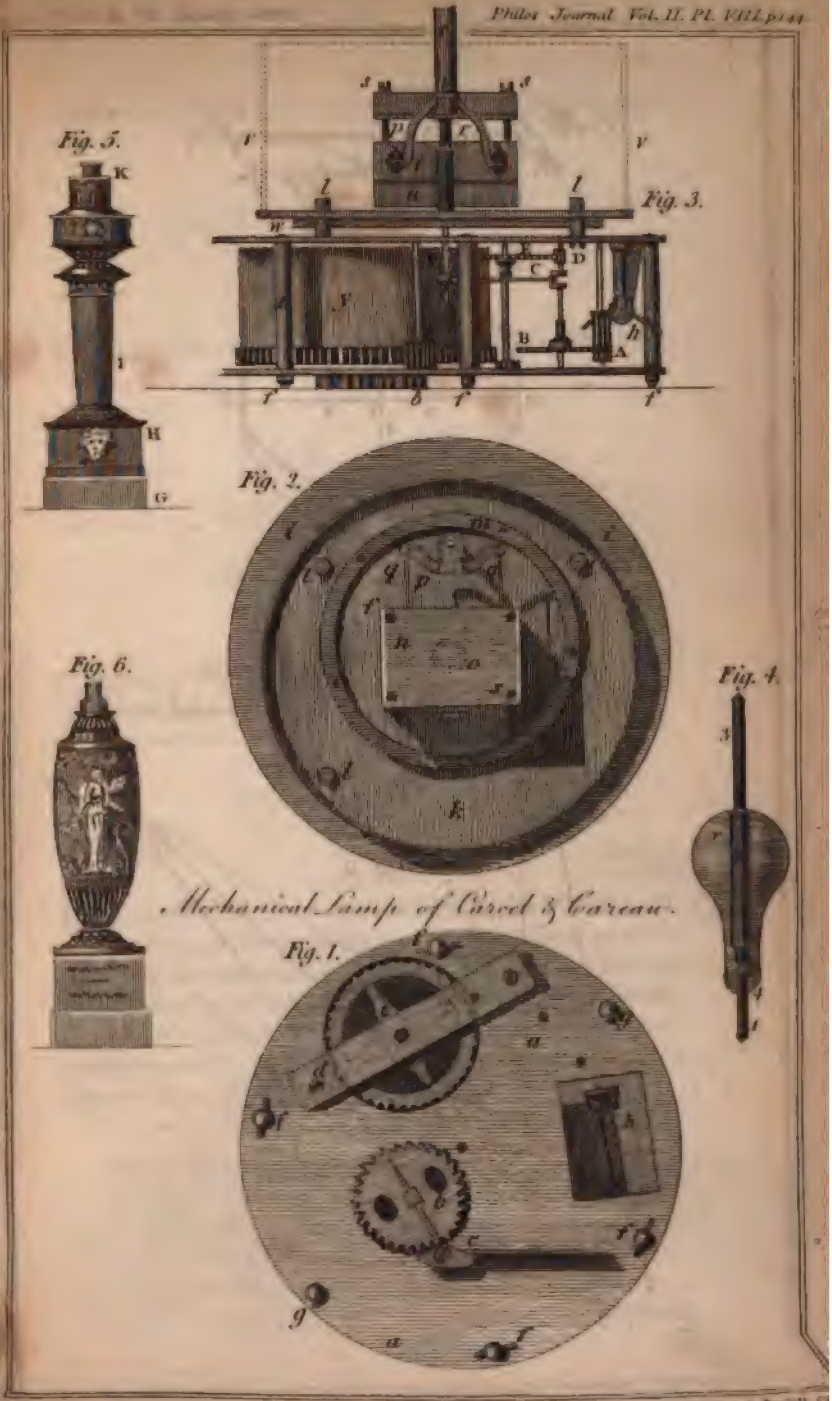


Fig. 5.
Fig. 3.
Fig. 2.
Fig. 6.
Fig. 4.
Mechanical Lamp of Carcel & Careau.
Fig. 1.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1802.

ARTICLE I.

Compofition of Writing Ink, poffeffing the permanent Colour, and other effential Properties, of the Ink ufed for Printing. In Letter from Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

Dalton, May 25, 1802.

IN the fecond volume of the Philofophical Journal, at p. 63, the preparation of indelible ink is announced. Such an article may juftly be confidered a defideratum. As I have for fome time directed my attention to the compofition of permanent ink, I fend you a fhort memoir on the fubject, which was drawn up previous to the perufal of the above-mentioned notice, and intended for a future communication. Preliminary addrefs.

THE welfare of individuals often depends very much on the teftimony of writing. An atramentous compofition, poffeffing a permanent colour on paper, and fuch a degree of infolubility, after deficcation, as not to fuffer any injury from expofure to humidity, or the application of fuch chemical pre- Permanent ink an important article for the prefervation of authentic eftimony in writing.

 parations

parations as totally eradicate the traces of common ink, would certainly be an article of confiderable value in the material compofition of thofe writings, or legal inftruments, which are intended to evince the tranfactions of the day, and to ratify the affairs of the future.

The invention of printing probably produced fome remiffnefs about the durability of ink.

The ancients were probably more interefted in the compofition of permanently coloured ink than the moderns. When every book was in manufcript, it would frequently be requifite to tranfcribe whole volumes, fometimes merely for the purpofe of procuring a copy, at other times for the prefervation of the work itfelf; in either cafe, before the writer begun a procefs fo tedious, it would be of confequence to felect fuch materials as were the moft durable, in order to protract the like neceffity, and alfo for the purpofe of enhancing the value of his labour. But fince the invention of printing, the neceffity of tranfcribing has been entirely fuperfeded; the manufcripts of modern authors after publication, have been reduced to little more value than objects of curiofity; and writers have been contented with fuch materials as are the cheapeft and eafieft to procure.

Ink with vegetable infufions and fulphate of iron,

not durable.

For a great number of years, the ink moft generally ufed by European writers has been the infufion of galls, and other aftringent vegetables containing gallic acid, rendered black by fulphate of iron, and thickened by the addition of a little gum or fugar. This compofition is well adapted to the common purpofes of writing and the difpatch of bufinefs, but its colour is liable to fade: a compofition of a more permanent colour is therefore defirable, for writings which are required to retain their primitive fignatures, and fuch as cannot be printed. Indeed, fince the difcovery of the method of totally difcharging the traces of common ink by the application of the oxigenated muriatic acid, more ferious confequences are to be apprehended from the univerfal ufe of the common atramentous fluid, than the decay of its colour from age; for it is well known, that while the fulphate of iron remains on the paper, the colour of the writing may be reftored, by wafhing the manufcript with frefh infufion of galls.

The colour may be reftored while the fulphate of iron remains on the paper. The moft ferious objection to its ufe is, that it may be totally difcharged.

Means of preventing this by the addition of pigments.

In the quarto feries of the Philofophical Journal (Vol. IV. p. 479), there are feveral ufeful receipts for compofing ink capable of refifting the oxigenated muriatic acid. The moft material difference, however, of the compofitions there re-

 commended

commended from thofe in common ufe, confifts in the addition of pigments producing an unchangeable colour upon paper.

Ink of a permanent colour may be eafily obtained, by fufpending various pigments in an aqueous fluid, by the intervention of gum, without the affiftance of the common ingredients; but fuch compofitions are liable to one of the greateft inconveniences, for the whole of the writing may be detached from the paper by wafhing the manufcript with water. Such ink, however, was frequently ufed by the ancients. Ancient method of compofing ink.

As a permanent colour is certainly a valuable requifite, it appears very promifing in fpeculation for the improvement of this indifpenfible article, according to the fimpleft, and perhaps the moft ancient method of compofition, to fubftitute, in place of the common mucilaginous fluid, as a compound vehicle for the diftribution and protection of the colouring matter, the folution of fome gum, or refinous fubftance, which can be diffolved in only a few liquids. After the diffipation of the thinner part of an atramentous compound properly formed with fuch a folution, the colouring fubftance will be left on the paper, combined with a fufficient quantity of tenacious matter to protect it from being injured by friction, or from being difcharged by the application of any fluid to which the writing may be expofed, without injuring the paper. Speculation for the improvement of this kind of ink.

Many of the more volatile kinds of oils may be ufed in writing, if reduced to a proper confiftence by the addition of gum or refin. Tolerable ink may be made by diffolving 30 grains of common refin in 90 grains of oil of turpentine, and tempering the folution with 17½ grains of lamp black, and 2½ of indigo. In a dry ftate, this compofition refifts the action of water, but not of fpirit. Such, indeed, will be the cafe with every compofition in which the colour is merely fufpended in the fluid, and attached to the paper, by a fubftance of eafy folubility; the application of the article which produced the fluidity of the ink, will again penetrate and foften the dry compound. Thofe compofitions which contain a tenacious matter, foluble in a few articles only, and at an high temperature, will be the leaft exceptionable. Ink with oil and refin. Inconvenience to which every compofition will be fubject when the colour is mechanically fufpended.

A more infoluble kind of matter than refin fhould be ufed. Copal will diffolve in only a few liquids; it appears well adapted to the purpofe of retaining a permanent colour upon the Copal is much fuperior to refin for the fufpenfion of the pigment;

the paper, if a vehicle can be found which will diffolve a fufficient quantity, and which, when the colouring matter is alfo added, fhall be fluent enough in writing.

but is not altogether free from inconvenience.

Oil of lavender will diffolve copal. I have made fome experiments with thefe articles, and the combination fucceeds fo well, that I have not been inclined to ufe any other. The only inconvenience to be apprehended from the ufe of copal in the compofition of ink, is, from its being foluble at a low temperature; but whether any other kind of tenacious matter of more difficult folubility, can be ufed conveniently in writing, I muft leave to future difcuffion. The folution of a fubftance which could only be diffolved in an high temperature, would certainly be the moft proper vehicle to prevent alteration; for, after the deficcation of the writing, it would be difficult to foften the ink in one part of a manufcript, without expofing the whole to a very injurious procefs.

Compofition of ink with oil of lavender, copal, and lamp black.

Ink may be compofed of oil of lavender, copal, and lamp black, according to the following proportions of the ingredients:

Take of oil of lavender, 200 grs. copal in powder, 25 grs. lamp black from $2\frac{1}{2}$ to 3 grs. With the affiftance of a gentle heat, diffolve the copal in the oil of lavender in a fmall glafs phial, and then mix the lamp black with the folution upon a marble flab, or other fmooth furface. Put the compofition into the bottle, and keep it excluded from the air. After a repofe of fome hours, the ink muft be well fhaken and ftirred with a piece of wire before it is ufed; if it be too thick, it muft be diluted with a little oil of lavender, oil of turpentine, or alcohol. The facility of writing with this compofition depends much on the quantity of the colouring matter. Three grains of lamp black to two-hundred and twenty-five of the folution of copal, producing ink of a full body of colour, and is nearly as much as the copal can protect from injury after the diffipation of the oil of lavender. Two grains and an half of lamp black, and half a grain of indigo, produces ink of a paler colour, but which may be diftributed upon the paper with the facility of common ink. A piece of fponge *, or other organized

* The cuftom of putting cotton in an inkftand has been difufed, more particularly, I think, fince an inkftand, invented by the late worthy and ingenious Samuel More, of the Society of Arts, was made

ganized fubftance, muft be ufed in the inkftand, chiefly for the purpofe of cleaning the pen.

Red ink with red fulphuret of mercury.

Red ink may be made by tempering the folution of copal with red fulphuret of mercury inftead of lamp black. The following proportions of the ingredients produce a red ink which writes very well:

The recipe.

Take of oil of lavender 120 grs. copal in powder, 17 grs. red fulphuret of mercury, 60 grs. Diffolve the copal in the oil of lavender, and then mix the fulphuret with the folution upon a fmooth furface.

A manufcript written with thefe compofitions will not be injured by the procefs of bleaching.

Both thefe compofitions poffefs a permanent colour, and other effential properties of the ink ufed in printing. The oil of lavender being diffipated with a gentle heat, the colour is left on the paper furrounded with copal, a fubftance infoluble in water, in fpirits, in acids, or alkaline folutions. A manufcript, written with thefe compofitions, may therefore be expofed to the procefs commonly ufed for reftoring the colour of printed books, without the fmalleft injury to the writing; and in this manner all interpolations with common ink may be removed.

As

made and brought into fafhion about thirty years ago, by the celebrated Wedgwood. Thefe changes appear to be both injurious. For the ink in the cotton is kept blacker by the fufpenfion of the atramentous part; and if no more ink be prefent than perfectly to fill the cotton, the pen will always receive a fluid black ink, and may be charged at pleafure by a greater or lefs gentle preffure at the time of taking up, or difcharged by lodging the point for a moment upon the cotton. It is alfo very eafy to regulate the oxigenation by the air, fo as to increafe the blacknefs without fuffering mouldinefs to come on, by the fimple expedient of turning the cotton upfide down every day. As the fibres of the cotton prevent the fluid from circulating as ufual by the change of temperature produced from evaporation, the interior mafs may be confidered as in a clofed veffel while not in ufe.

Mr. More's fountain inkftand, at prefent fo univerfally in ufe, is certainly very inconvenient. The ink, it is true, is kept *in a clofed veffel*; but its colouring matter is at full liberty to fubfide, and the confumer is obliged to fill his pen from the muddy bottom inftead of the furface; and what is ftill worfe, the conical veffel into which the ink flows, is fubject to all the evils of evaporation and mouldinefs, fo as moft frequently to afford an adhefive and clogging fluid to the pen. W. N.

Source of apprehenfion. The oil of lavender will foften the dry compofition, but the writing cannot be eafily eradicated.

As copal is foluble in feveral of the effential oils, it may be expected that the application of thefe will do much injury to the writing. Such an expectation is well founded. If the writing be rubbed with a fmooth furface dipped in oil of lavender, much of the colouring matter will be difengaged, and diftributed more diffufely upon the paper; but, as fome of it will have penetrated the interior parts of the paper along with the copal, it will be extremely difficult to obliterate the traces of the pen without the erafement being perceptible. This is the principal fource of apprehenfion, but I know of no method of obviating the danger. The perufal of a memoir on the nature and preparation of drying oils, &c. by Mr. Sheldrake (Philof. Journal, 8vo. Vol. I. p. 259), fuggefts amber as a proper article for the compofition of ink, if enough of it can be diffolved in any fluid fufficiently thin for the purpofe of writing. The more difficulty there is in effecting the folution of any tenacious fubftance we ufe, the better, provided it can then be employed without inconvenience.

Amber a proper fubftance if it could be ufed.

I am, Sir,

Yours refpectfully,

WILLIAM CLOSE.

II.

Experiments and Obfervations on the Vegetation of Plants, which fhew that the common Opinion of the Amelioration of the Atmofphere, by Vegetation in Solar Light, is ill founded. By JAMES WOODHOUSE, *M. D. Profeffor of Chemiftry in the Univerfity of Pennfylvania, &c.*

To Mr. NICHOLSON.

SIR,

Pater Nofter Row, May 27, 1802.

Introductory letter.

I INCLOSE for the Philofophical Journal, the refults of various experiments, made in Philadelphia in the year 1801, upon the feeds, leaves, &c. of a variety of plants, which feem to prove, that growing vegetables, contrary to an opinion almoft univerfally adopted, do not purify atmofpherical air; and that whenever

whenever they appear to afford oxigenous gas, it is by devouring the coal of carbonic acid gas for food, and leaving its oxygen in the form of pure air.

I have the honour to be,
Dear Sir,
With the greateft refpect,
Your moft obedient,
And very humble fervant,
JAMES WOODHOUSE.

First. *Of the Effects produced by the Germination of Seeds in atmofpherical Air.*

On the 3d of June, twelve feeds of zea maiz were planted in earth, and confined over water in a glafs veffel, in feventy ounce meafures of atmofpherical air of the purity of 100, and often expofed to the light of the fun. On the 12th, the corn had vegetated, and was from two to five inches high. The air being examined at this time, by throwing up one meafure of it, over lime water, in an eudiometer, gave $\frac{3}{100}$ parts of carbonic acid air. Another meafure, after being freed from the fixed air, and mixed with an equal meafure of nitrous air, produced an abforption of $\frac{10}{100}$. On the 19th, the corn having grown confiderably, and the air being tried again, no carbonic acid gas appeared, and the purity was the fame as at firft. On the 23d, the plants died, and the airs were found to confift of $\frac{5}{100}$ fixed, and $\frac{95}{100}$ azotic gas.

Germination of feeds in atmofpheric air.

Similar experiments were made with the feeds of apium petrofelinum, lactuca fativa, cucurbita citrullus, phafeolus fativus, fifymbrium, and raphanus fativus, and with the fame refult.

The atmofpherical air, in thefe experiments, appears to be reduced in purity, by its oxigen uniting to the coal of the cotyledons of the feed, or to that of fome animal or vegetable matter contained in the earth in which the feeds are planted, or to that of fome decayed portion of the living leaves.

The air loft oxigen by uniting to carbon; afterwards it became more pure; and laftly its oxigen was totally abforbed.

Ingenhouz, Humbold, and Thomfon, have obferved, that foils have the property of abforbing oxigen; but as it cannot be proved that any pure earth, or mixture of earths, render atmofpherical air impure, it is certainly more philofophical to afcribe the impurity of the air to the formation of the carbonic acid, the bafe of which generally exifts in all foils.

The effect of veg. mould on air moft probably from its carbon.

II. *Of*

II. *Of the Effects produced by the Growth of Plants in atmospherical Air.*

Growth of plants in atmos. air.

On the 27th of May, twelve plants of persicasia polygonum, two inches high, growing in earth, were confined in a glass vessel in fifty-two ounce measures of atmospherical air, of the purity of 100, and often exposed to the influence of solar light. On the 4th of June, they had increased about two inches in height. The air being examined at this time, was found to contain $\frac{1}{100}$ parts of carbonic acid gas, and to be reduced in purity to 80. Several young plants of rhaphanus sativus, latura stramonium, phytolacca decandra, zea maiz, phaseolus sativus, sidum telephium, amaranthus hyboidus, cucurbita citrullus, sirymbrium, and lactuca sativa, were also separately confined in from forty to eighty ounce measures of atmospherical air, which was examined at various times, from one hour to thirty days, after the plants had been placed in it. Carbonic acid gas was generally formed, and whenever this circumstance happened, the purity of the air was diminished.

They produced carbonic acid gas, and diminished the purity of the air.

Growth of plants in oxigen diminish its purity by carbonic acid.

Many of the same kind of vegetables were also confined in forty ounce measures of oxigenous gas, which had been well washed in lime water, and the purity of the air was very generally lessened, fixed air being generated. They turned of a white or yellow colour, and soon died, after being placed in atmospherical air.

In confined plants the decaying parts afford carbon and form acid, which the living plant decomposes.

The same effects are produced by the growth of plants as by the germination of seeds in common air, and by the same causes. If the leaves are confined a considerable time, part of them decay, and the coal of the dead portion, uniting with the oxigen of the atmospheric air, generates carbonic acid. This acid is decomposed by the living leaf. Its coal is abstracted, while its oxigen is left in the form of pure air.

But when the formation is quicker than the decomposition, the plant dies.

When the oxigen unites to the coal of the animal or vegetable matter of the soil in which the plants vegetate, or to the coal of the decayed parts of the leaves, and makes fixed air quicker than the living parts can decompose it, the plants will speedily die.

When the soil contains but little of organized remains, the included plant will live much longer.

When a plant in perfect health, growing in a soil which contains little vegetable or animal matter, is confined in atmospherical air, it will live a long time, without producing any change in it. Many of the vegetables which were the subjects of these experiments,

experiments, did not affect the air in five days: some diminished its purity in three hours; and others altered it in a most slow and gradual manner, causing little change in it in twenty days.

III. *Of the Effects produced by the Leaves of Plants in atmospherical Air impregnated with Carbonic Acid Gas, and exposed to the Light of the Sun.*

Leaves exposed to solar light in a mixture of atmospheric and carbonic acid gas.

A handfull of the leaves of mimosa virgata, euphorbia picta, digitalis purpurea, franklinia altamaha, asparagus officinalis, coryllus avellana, rhus glabrum, aristotochia siphoe, and periploca græca, were separately exposed seven hours to the light of the sun, in thirty-six ounce measures of atmospheric air, impregnated with four ounce measures of carbonic acid gas, from the carbonate of lime and sulphuric acid. The fixed air disappeared, and the atmospheric air was so much increased in purity, as to devour two measures of nitrous air.

The carbonic acid disappeared, and the proportion of oxigen in the mixture was augmented.

The leaves of these plants, kept over night in the same air, gave carbonic acid gas in the morning; and its purity, in every instance, was considerably diminished.

In the dark the leaves produced carbonic acid gas.

The leaves of mimosa virgata and amygdalus persica, were also separately exposed nine hours to the influence of solar light, in forty ounce measures of atmospherical air, in which fixed air had been formed by leaving a fungus to putrefy it. The carbonic acid gas disappeared, and the purity of the atmospherical air was increased from 50 to 80.

Other leaves exposed to light, with the former result.

IV. *The following Tables will shew the Quantity and Purity of oxigenous Gas, obtained by exposing a small Handful of the Leaves of Plants to the Light of the Sun, in forty Ounce Measures of Pump Water.*

Table of experiments on leaves exposed to solar light under pump water.

This water was taken from a well sunk within a few yards of a necessary, from which it was impregnated with carbonic acid gas, as appeared from an analysis. The leaves were separately exposed in glasses arranged near each other, and from eight to thirteen comparative experiments were made at one time.

Leaves

Leaves of	Carbonic Acid Gas in 100 Parts.	Oxygenous Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Do. with two Measures.	Do. with three Measures.	State of the Thermometer.	Time when expofed.
Alcea rofea - - -	From 8 to 9 Parts.	19½	122	146	96	105° to 110° of Fahrenheit.	July 2, 1802.
Zea maiz - - -		16	116	140	54		The day was very clear.
Amaranthus fpinofa -		15	120	140	68		
Meliffa officinalis - -		13	120	130	50		
Hyfopus - - -		16	120	138	70		
Convolvulus purpureus -		8	110	110	0		
Malva rotundifolia - -		17	120	140	86		
Lavendula - - -		16	118	130	55		
Rofa centifolia - -		15	112	130	46		
Mirabilis dichtoma - -		16	110	130	40		
Convolvulus purpureis -	From 8 to 10 Parts.	13	110	120	40	100° to 115°.	July 3.
Anthemis nobilis - -		12	114	120	32		Day clear.
Hibifcus Syriacus - -		12	118	130	65		
Polygonum aviculare -		18	114	130	50		
Amygdalus Perfica -		10	114	112	12		
Pyrus malus - - -		16	116	120	20		
Platanus occidentalis -		12	120	140	20		
Tilia Americana - -		10	120	138	40		

Leaves

Leaves of	Carbonic Acid Gas in 100 Parts.	Oxygenous Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Do. with two Measures.	Do. with three Measures.	State of the Thermometer.	Time when exposed.
Siriodendron tulipifia -	8.	14	112	120	25	105° to 110°.	July 4, 1801. Day generally clear.
Populus dilatata - -		14	110	132	60		
Æfculus pavia - -		13	110	130	60		
Apium petrofelinum -		12	115	132	55		
Convolvulus purpureus -		5	120	120	30		
Helianthus annuus - -		13	112	132	62		
Ruta graveolens - -		10	120	130	40		
Trifolium paluftri - -		13	120	140	55		
Datura ftrammonium -		14	112	130	80		
Hyfopus - - -		7	112	132	65		
Blattari verbafcum - -	8 & 9.	12	112	130	45	95°.	July 5. Day clear and cloudy. Twelve ounce meafures of this oxigenous air, after being wafhed in lime water, to free it from the carbonic acid gas, being expofed to a mixture of iron filings and fulphur, were found to confift of eight ounce meafures of oxigenous, and four of azotic gas.
Chelidonium majus - -		18	112	136	80		
Chryfanthimum Indicum -		14	120	142	63		
Acer glaucum - -		14	120	139	63		
Phytolacca decandra -		14	120	140	80		
Antirrhinum linaria - -		18	120	140	65		
Arctum cappa - -		12	120	140	53		
Syringa vulgaris - -		8	120	132	40		
Helianthus altiffimus -		12	120	140	55		
Polygonum Perficana -		12	120	140	80		
Cercis Canadenfis - -		12	120	140	60		
Sonicera caprifolium -		12	120	140	60		
Diofpyros Virginiana -	8 to 10.	10	120	120	30	90° to 110°.	July 6. Day clear and cloudy. Thefe leaves were gathered in the evening, and kept until morning, in a cool place.
Franklinia altamaha -		10	120	102	0		
Chionanthus Virginica -		8	120	100	0		
Arundo gigantia - -		10	120	130	32		
Afclepias Syriaca - -		9	120	80	0		
Annona triloba - -		10	120	130	40		
Magnolia glauca - -		10	110	102	0		
——— tripetala - -		16	116	130	40		
Xanthoriza tinctoria -		8	120	130	50		
Conferva civularis - -		10	120	120	30		
Alcea rofea - - -		5	110	70	0		
Sophora indica - -		7	110	80	0		
Laurus faffafras - -		10	120	92	0		

We

Opinion that plants supply oxigen to the atmosphere,

We are indebted to Dr. Prieftley for the difcovery, that plants expofed to light yield oxigenous air; and ever fince it has been made, an opinion has been adopted, that growing vegetables fupply the oxigenous portion of atmofpherical air, of which there is a conftant confumption, by combuftion, fermentation, refpiration, and the calcination of metals.

ill founded;

If this fubject is attentively examined, it will be found that plants have no effect in rendering the air of the atmofphere pure.

becaufe they afford none unlefs carbonic acid be prefent. Experiments of Prieftley in proof;

Firft. Whenever oxigenous gas has been obtained from vegetables, carbonic acid gas has been prefent.

Dr. Prieftley expofed plants to atmofpheric air, in which fpirit of wine and wax and tallow candles had burned out; to air which had been vitiated by the death or putrefaction of mice and fifhes, and to air which had been frequently taken into his lungs. He alfo obferved, that there was a flower and lefs production of air from rain and diftilled, than from pump and ftagnant water.

and of the author; tabulated.

The difference between the quantity and quality of the gas, obtained from river water and the fame water impregnated with carbonic acid, by expofing plants in it to the influence of folar light, will be feen by the following table:

Leaves

Leaves of	Carbonic Acid Gas, in 100 Parts.	Quantity of Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Do. with two Measures.	Do. with three Measures.	State of the Thermometer.	Time when exposed.
Siriodendron tulipifera -	None.	From half a Drachm to one Drachm.	55			110°.	July 7, 1801. Day very clear. The leaves were exposed in the water of the river Schuyltril.
Cercis Canadensis - -			70				
Tilia Americana - -			50				
Salix Babylonica - -			32				
Polygonum Persicaria -			30				
Phytolacca decandra -			94				
Platanus occidentalis -			90				
Alcea rosea - - -			84				
Helianthus annuus - -			83				
Amygdalus Persica - -			82				
Conferva fontinalis - -			80				
Zea maiz - - -			75				
Acer glaucum - -			90				
Seriodendra tulipifiera -	In some of the vessels none, in others, from 5 to 10 Parts.	6	120	130	40	110°.	July 8, 1801. Day a little hazy, although the sun shone constantly. The leaves of the same plants, in the same river water, impregnated with four quarts of the water, saturated with carbonic acid gas, from carbonate of lime and the sulphuric acid.
Cercis Canadensis - -		6	116	124	30		
Tilia Americana - -		5	110	160	0		
Salix Babylonica - -		5	120	100	0		
Polygonum Persicaria -		10	120	140	70		
Phytolacca decandra -		6	120	140	42		
Platanus occidentalis -		3	110	60	0		
Alcea rosea - - -		6	120	132	40		
Helianthus annuus - -		10	120	110	50		
Amygdalus Persica - -		6	120	138	40		
Conferva fontinalis - -		4	120	134	50		
Zea maiz - - -		4	115	125	20		
Acer glaucum - -		6	120	140	50		

It appears from this table, that the leaves of thirteen different plants, separately exposed in forty ounce measures of the water of the river Schuyltrill, produced about ten drachm measures of air, the principal part of which was azotic gas; whereas the same kind of leaves, exposed in the same quantity of the same water, impregnated with carbonic acid, yielded seventy-seven drachm measures of oxigenous air, of a very high degree of purity.

Count

Count Rumford's experiments to obtain oxigen from water by folar light.

Count Rumford made an attempt, in the year 1787, to overthrow the doctrine of the purification of the air by plants. His arguments were, that leaves confined in water were in unnatural circumftances, and that pure air could be obtained from other bodies, as fine fpun glafs, raw filk, common cotton, and that of the poplar tree, expofed in water to the light of the fun *.

Remarks on Prieftley's

The ingenious author of Phytologia alfo fays, it may be fufpected that, in many of the experiments of Prieftley and Ingenhouz, the production of vital air might be fimply owing to the action of the fun's light on the water in which the vegetables were immerfed, like that from the filk in the experiments of Count Rumford; and that the fine points or fharp edges of thefe bodies, contributed only to facilitate the liberation of it when expofed to the fun fhine, which thus difoxigenated the water by their united effect.

and Count Rumford's experiments.

The experiments of Count Rumford are far from being fatisfactory. Thirty grains of raw filk, at the end of three days, yielded him but 3¼ cubic inches of air, and fometimes four days elapfed before a fufficient quantity could be collected for an experiment.

Direct experiments

In order to find how much air could be obtained from the "fine points or fharp edges" of certain bodies *acting* upon water, the following fubftances were expofed one day to the action of folar light, in forty ounce meafures of pump water.

with fibrous bodies, which gave air lefs in quantity and purity than leaves.

Filaments of afbeftos, baked horfe-hair, common cotton, and that of the afclepias Syriaca, the flower panicles of rhus cotinus, the fine hairy plumes of climatis crifpa, the fpikes of panicum glaucum, and charcoal in powder. From each of thefe fubftances, from two to four drachm meafures of pure air were obtained, which devoured nearly two meafures of nitrous air; confequently it was lefs pure than that procured from leaves expofed in the fame water. There was alfo a much fmaller quantity of it; for from eight to nineteen drachm meafures may be obtained in a few hours, by immerfing the leaves of any plant in the fame water, and expofing them to folar light.

Other fources of air.

Some water, without any mixture, will yield oxigenous gas by the combined action of light and heat; and many fubftances placed in water, appear to act merely by raifing its temperature.

* Tranfactions of the Royal Society for 1787.

The

The green vegetable matter, which forms on all bodies, immerfed a confiderable time in water, might alfo have been one of the fources of pure air, in fome of the experiments of Count Rumford.

Secondly. Many philofophers fuppofe, that vegetables yield oxigenous gas by the decompofition of water. Its hidrogen is faid to enter into plants, while its oxigen is fet at liberty in the form of pure air.

Plants do not decompofe water; for they do not operate in pure water.

If this opinion was true, oxigenous gas fhould be obtained by expofing leaves in boiled, rain, diftilled, river, or lime water, but this cannot be done.

Thirdly. Some fuppofe that vegetables give oxigenous air to animals, and that the latter yield them azotic gas in return, which they devour for food.

Plants do not (as has been fuppofed) emit oxigen and abforb azote;

If this hypothefis were juft, atmofpheric air would be increafed in purity by confining leaves in it when it contained no fixed air; and its purity might alfo be increafed, after being previoufly diminifhed, by an additional quantity of azotic air, in the fame manner.

A handful of the leaves of euphorbia picta, nicotiana tobacco, buxus vulgaris, cinna glauca, mimofa julibrefcens, jaxus procumbens, coryllus avellana, Herculea fœtida, malva crifpa, pinus ftrobus, colutea arborefcens, and epilobium, were feparately expofed four hours to the light of the fun, in forty ounce meafures of atmofpheric air, and its purity was found to be neither increafed nor diminifhed. After they had remained fixteen hours in the air, no effect was produced on it. The leaves were frefh gathered, and no decay could be obferved upon any part of them.

for frefh leaves do not affect atmofpheric air,

When leaves are plucked promifcuoufly, and are placed in atmofpheric air either in the day or night, they diminifh its purity. Wherever a leaf is perforated, and this is very generally done by infects, let the perforation be ever fo fmall, the part decays, and the coal of this decayed part uniting to the oxigen of the atmofpheric air, generates carbonic acid, which leffens its purity.

though wounded or decaying leaves do.

The

Experiments in folar light;

The following table fhews the effect of the leaves of plants gathered promifcuoufly, expofed five hours to the light of the fun, in forty ounce meafures of atmofpherical air, at a temperature of 75° of Fahrenheit.

A fmall handful of the	Fixed Air.	Atmofpheric Air of the fecond And. of 100.
Leaves of Datura ftrammonium - -	3	96
Rododendron maximum -	5	87
Apium petrofelinum - -	4	86
Anthemis nobilis - -	0	100
Sophora auftralis - -	2	95
Sedum telephium - -	0	100
Amaranthus hybridus - -	10	70

In the dark.

The following table will fhew the effects produced in one night, on forty ounce meafures of atmofpheric air of the purity of 100, by a fmall handful of leaves gathered promifcuoufly from a variety of plants.

	Fixed Air.	Atmof. Air.
Leaves of Ilex aquifolium - - -	5	88
Juniperus officinalis - -	4	93
Berberis vulgaris - -	2	86
Franklinia alatamaha - -	3	85
Rododendron maximum -	1	95
Annona triloba - - -	2	88
Buxus vulgaris - - -	2	90
Pinus ftrobus - - -	2	88
Mitchella repens - -	0	100
Arclepias Syriaca - -	5	86
Hamamelis Virginia - -	0	100
Bignonia radicans - -	3	77
Xanthoriza tinctoria - -	1	94
Magnolia tripetala - -	5	67
Kalmia latifolia - - -	2	85
Pinus picea - - -	3	80
Siriodendron tulipifera -	10	65

According to fome philofophers, carbonic acid gas is fecreted by certain vegetables in the night; but as the quantity of this air obtained is always in proportion to the decayed parts of plants,

plants, and to the temperature to which they are fubjected, it appears more rational to afcribe the generation of it to the coal of the decayed parts uniting with the oxigen of the air in which they are placed.

Leaves expofed to funfhine in a mixture of atmofpheric air and azote, produced no effect.

To determine whether plants would abforb or devour azotic gas, eight ounce meafures of this air were mixed with thirty-two ounce meafures of atmofpheric air, fo that its purity was reduced from 100 to 91. A handful of the leaves of euphorbia picta and coryllus avellana were feparately confined in forty ounce meafures of this air, and expofed to the influence of a bright folar light five hours. No carbonic acid gas was generated, and the purity of the air was exactly the fame as when firft tried. No decayed portion could be obferved upon thefe leaves.

Leaves do not purify the atmofphere by decompofing its carbonic gas; becaufe the quantity of this gas is very minute,

As it is acknowledged that the leaves, ftems, and roots of plants, feparate the oxigen from carbonic acid, it may be faid, that the oxigenous portion of atmofpheric air is fupplied by the decompofition of this gas, as it is always found in the atmofphere, and often in water in which vegetables grow.

The quantity of carbonic acid gas in atmofpheric air, is reckoned to be about one part in an hundred. It muft, however, vary in different places. We would expect to find the moft of it in cities, where it is formed by combuftion, refpiration, fermentation, and putrefaction. If one meafure of the air of any large city is thrown up over lime water in an eudiometer, no milky appearance will be produced, fo that the quantity of carbonic acid in this air muft be extremely fmall. As this gas is alfo feized upon by alkalis, earths, and metals, and abforbed by water, the quantity floating in the atmofphere may be lefs than one part in ten thoufand.

and plants deteriorate the air much more.

When we confider likewife, that the oxigen is never feparated from the carbonic acid by leaves, but when they are expofed, in contact with it, to the light of the fun; and that every perforation made in a living leaf, however minute, by an infect, caufes the part to decay, and abforb oxigen by day and by night; and that, in the autumn in fome countries, all leaves fall on the ground, ferment or putrify, and thus diminifh the purity of common air; and that the petals and fruit of vegetables have the fame effect, we muft pronounce, that the oxigenous portion of atmofpheric air cannot be fupplied by vegetation.

Air bladders of various plants contain air worse than that of the atmosphere.

Dr. Darwin suppoſes, that the air in the air bladders of vegetables ſerve to oxigenate the ſeed. The air of the air bladders of cardioſpermum halicacabum, ſtaphylia trifoliata, colutea arboreſcens, and ſophora auſtralis being examined, was found to be a little worſe than the air of the atmoſphere.

III.

On the Theory of Light and Colours. By Thomas Young, *M. D. F. R. S. Profeſſor of Natural Philoſophy in the Royal Inſtitution.*

(Concluded from page 90.)

PROPOSITION IV.

Prop. IV. Partial reflection of undulations at the confine of mediums differing in denſity.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Denſities, a partial Reflection takes place, proportionate in Force to the Difference of the Denſities.

THIS may be illuſtrated, if not demonſtrated, by the analogy of elaſtic bodies of different ſizes. " If a ſmaller elaſtic body ſtrikes againſt a larger one, it is well known that the ſmaller is reflected more or leſs powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light paſs from a rarer to a denſer ſtratum of ether; and frequently an echo when a ſound ſtrikes againſt a cloud. A greater body ſtriking a ſmaller one, propels it, without loſing all its motion: thus, the particles of a denſer ſtratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting ſubſtance with equal force; and thus a reflection is always ſecondarily produced, when the rays of light paſs from a denſer to a rarer ſtratum." (Phil. Tranſ. for 1800, p. 127.) But it is not abſolutely neceſſary to ſuppoſe an attraction in the latter caſe, ſince the effort to proceed would be propagated backwards without it, and the undulation would be reverſed, a rarefaction returning in place of a condenſation; and this will perhaps be found moſt conſiſtent with the phenomena.

PROPOSI-

PROPOSITION V.

When an Undulation is tranſmitted through a Surface terminating different Mediums, it proceeds in ſuch a Direction, that the Sines of the Angles of Incidence and Refraction are in the conſtant Ratio of the Velocity of Propagation in the two Mediums.

Prop. V. Law of refraction of undulations tranſmitted through the confine of different mediums.

(Barrow, Lect. Opt. II. p. 4. Huygens, *de la Lum*. cap. 3. Euler, *Conj. Phyſ.* Phil. Tranſ. for 1800, p. 128. Young's Syllabus. Art. 382.)

Corollary 1. The ſame demonſtrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condenſed air, that the ratio of the difference of the ſines varies ſimply as the denſity. Hence it follows, by Schol. I. Prop. I. that the exceſs of the denſity of the ethereal medium is in the duplicate ratio of the denſity of the air; each particle co-operating with its neighbours in attracting a greater portion of it.

PROPOSITION VI.

When an Undulation falls on the Surface of a rarer Medium, ſo obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence. (Phil. Tranſ. for 1800, p. 128.)

Prop. VI. Total reflection of undulation falling obliquely on the ſurface of a rarer medium.

Corollary. This phenomenon tends to prove the gradual increaſe and diminution of denſity at the ſurface terminating two mediums, as ſuppoſed in hypotheſis IV; although Huygens has attempted to explain it ſomewhat differently.

PROPOSITION VII.

If equidiſtant Undulations be ſuppoſed to paſs through a Medium, of which the Parts are ſuſceptible of permanent Vibrations ſomewhat ſlower than the Undulations, their Velocity will be ſomewhat leſſened by this vibratory Tendency; and, in the ſame Medium, the more, as the Undulations are more frequent.

Prop. VI. Undulations are retarded by paſſing through a medium vibrating with leſs frequency.

For, as often as the ſtate of the undulation requires a change in the actual motion of the particle which tranſmits it, that change will be retarded by the propenſity of the particle to continue its motion ſomewhat longer: and this retardation will be more frequent, and more conſiderable, as the diffe-

 rence

rence between the periods of the undulation and of the natural vibration is greater.

Doctrine of heat consisting in vibrations.

Corollary. It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum. (Newt. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count Rumford, Professor Pictet, and Mr. Davy, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.

Suppose the parts of all bodies to vibrate;

Let us suppose that these vibrations are less frequent than those of light; all bodies therefore are liable to permanent vibrations slower than those of light; and indeed almost all are liable to luminous vibrations, either when in a state of ignition, or in the circumstances of solar phosphori; but much less easily, and in a much less degree, than to the vibrations of heat.

then the more frequent luminous undulations will be most retarded. Blue light will be most refracted, and *radiant heat least of all,*

It will follow from these suppositions, that the more frequent luminous undulations will be more retarded than the less frequent; and consequently, that blue light will be more refrangible than red, and radiant heat least of all; a consequence which coincides exactly with the highly interesting experiments of Dr. Herschel. (Phil. Transf. for 1800, p. 284.) It may also be easily conceived, that the actual existence of a state of slower vibration may tend still more to retard the more frequent undulations, and that the refractive power of solid bodies may be sensibly increased by an increase of temperature, as it actually appears to have been in Euler's experiments. (Acad. de Berlin. 1762. p. 328.)

and refractive power will increase with the temperature.

Scholium. If, notwithstanding, this proposition should appear to be insufficiently demonstrated, it must be allowed to be at least equally explanatory of the phenomena with any thing that can be advanced on the other side, from the doctrine of projectiles; since a supposed accelerating force must act in some other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of assigning a mechanical cause. Mr. Short, when he found by observation the equality of the velocity of light of all colours, felt the objection so forcibly, that he immediately drew an inference from it in favour of the undulatory system. It is assumed in the proposition,

This position explains the phenomenon at least as well as the projectile hypothesis.

propofition, that when light is difperfed by refraction, the corpufcles of the refracting fubftance are in a ftate of actual alternate motion, and contribute to its tranfmiffion; but it muft be confeffed, that we cannot at prefent form a very decided and accurate conception of the forces concerned in maintaining thefe corpufcular vibrations.

PROPOSITION VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint Effect is a Combination of the Motions belonging to each.

Prop. VIII. Undulations that coincide in direction will combine.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwife than by uniting their motions, fo that the joint motion may be the fum or difference of the feparate motions, accordingly as fimilar or diffimilar parts of the undulations are coincident.

I have, on a former occafion, infifted at large on the application of this principle to harmonics; (Phil. Tranf. for 1800, p. 130.) and it will appear to be of ftill more extenfive utility in explaining the phenomena of colours. The undulations which are now to be compared are thofe of equal frequency. When the two feries coincide exactly in point of time, it is obvious that the united velocity of the particular motions muft be greateft, and, in effect at leaft, double the feparate velocities; and alfo, that it muft be fmalleft, and if the undulations are of equal ftrength, totally deftroyed, when the time of the greateft direct motion belonging to one undulation coincides with that of the greateft retrograde motion of the other. In intermediate ftates, the joint undulation will be of intermediate ftrength; but by what laws this intermediate ftrength muft vary, cannot be determined without further data. It is well known that a fimilar caufe produces in found, that effect which is called a beat; two feries of undulations of nearly equal magnitude co-operating and deftroying each other alternately, as they coincide more or lefs perfectly in the times of performing their refpective motions.

Effects when they are of equal frequency.

The *beat* in found.

COROLLARY I. *Of the Colours of ftriated Surfaces.*

Boyle appears to have been the firft that obferved the colours of fcratches on polifhed furfaces. Newton has not noticed

Colours of ftriated furfaces explained,

ticed them. Mazeas and Mr. Brougham have made ſome experiments on the ſubject, yet without deriving any ſatisfactory concluſion. But all the varieties of theſe colours are very eaſily deduced from this propoſition.

by the greater length of ray (or line of undulation) from the depreſſed portion of ſurface,

Let there be in a given plane two reflecting points very near each other, and let the plane be ſo ſituated that the reflected image of a luminous object ſeen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with reſpect to both points, conſidering them as capable of reflecting in all directions. Let one of the points be now depreſſed below the given plane; then the whole path of the light reflected from it, will be lengthened by a line which is to the depreſſion of the point as twice the coſine of incidence to the radius. Fig. 2. Plate VI.

which will occaſion alternations of intenſity and remiſſion in the compound undulation; accordingly as the depreſſion cauſes the undulations of the ſucceeding ray to coincide with or oppoſe thoſe of the preceding ray.

If, therefore, equal undulations of given dimenſions be reflected from two points, ſituated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depreſſed point will ſo interfere with the reflection from the fixed point, that the progreſſive motion of the one will coincide with the retrograde motion of the other, and they will both be deſtroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again deſtroyed; and thus for a conſiderable number of alternations: and, if the reflected undulations be of different kinds, they will be variouſly affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

Experiment by parallel ſtrokes;

In order that the effect may be the more perceptible, a number of pairs of points muſt be united into two parallel lines; and, if ſeveral ſuch pairs of lines be placed near each other, they will facilitate the obſervation. If one of the lines be made to revolve round the other as an axis, the depreſſion below the given plane will be as the ſine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this ſine.

ſuch as the fine micrometers of *Coventry* at the

The beſt ſubjects for the experiment are Mr. Coventry's exquiſite micrometers; ſuch of them as conſiſt of parallel lines drawn

drawn on glafs, at the diftance of one five hundredth of an inch, are the moft convenient. Each of thefe lines appears under a microfcope to confift of two or more finer lines, exactly parallel, and at the diftance of fomewhat more than a twentieth of that of the adjacent lines. I placed one of thefe fo as to reflect the fun's light at an angle of 45°, and fixed it in fuch a manner, that while it revolved round one of the lines as an axis, I could meafure its angular motion; and I found, that the brighteft red colour occurred at the inclinations $10\frac{1}{4}°$, $20\frac{1}{4}°$, 32°, and 45°; of which the fines are as the numbers 1, 2, 3, and 4. At all other angles alfo, when the fun's light was reflected from the furface, the colour vanifhed with the inclination, and was equal at equal inclinations on either fide.

diftance of one five hundredth of an inch.

When the angular pofition of thefe is varied they produce colours,

This experiment affords a very ftrong confirmation of the theory. It is impoffible to deduce any explanation of it from any hypothefis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a ftriking analogy between this feparation of colours, and the production of a mufical note by fucceffive echoes from equidiftant iron palifades; which I have found to correfpond pretty accurately with the known velocity of found, and the diftances of the furfaces.

which ftrongly and exclufively confirm the theory.

It is analogous to the mufical echo from iron rails.

It is not improbable that the colours of the integuments of fome infects, and of fome other natural bodies, exhibiting in different lights the moft beautiful verfatility, may be found to be of this defcription, and not to be derived from thin plates. In fome cafes, a fingle fcratch or furrow may produce fimilar effects, by the reflection of its oppofite edges.

Verfatile colours of fome infects, &c. probably of this nature.

COROLLARY 2. *Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting furfaces, the partial reflections coincide perfectly in direction; and, in this cafe, the interval of retardation, taken between the furfaces, is to their diftance as twice the cofine of the angle of refraction to the radius. For, in Fig. 3, Plate VI. drawing A B and C D perpendicular to the rays, the times of paffing through B C and A D will be equal, and D E will be half the interval of retardation; but D E is to C E as the fine of D C E to the radius. Hence, that D E may be conftant, or that the fame colour may be reflected, the thicknefs C E muft vary as the fecant of the angle of refraction C E D: which agrees

Colours of thin plates explained

by the greater length of ray reflected from the pofterior furface, which muft produce an effect

ſimilar to that of the ſtriated ſurfaces; namely colours,

agrees exactly with Newton's experiments; for the correction is perfectly inconſiderable.

Let the medium between the ſurfaces be rarer than the ſurrounding mediums; then the impulſe reflected at the ſecond ſurface, meeting a ſubſequent undulation at the firſt, will render the particles of the rarer medium capable of wholly ſtopping the motion of the denſer, and deſtroying the reflection, (PROP. IV.) while they themſelves will be more ſtrongly propelled than if they had been at reſt; and the tranſmitted light will be increaſed. So that the colours by reflection will be deſtroyed, and thoſe by tranſmiſſion rendered more vivid, when the double thickneſſes, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thickneſſes the effects will be reverſed; according to the Newtonian obſervations.

which will be tranſmitted and reflected by turns if the thickneſs gradually vary, as in the Newtonian rings.

If the ſame proportions be found to hold good with reſpect to thin plates of a denſer medium, which is indeed not improbable, it will be neceſſary to adopt the corrected demonſtration of PROP. IV. but, at any rate, if a thin plate be interpoſed between a rarer and a denſer medium, the colours by reflection and tranſmiſſion may be expected to change places.

Breadth and duration of the colorific undulations determined,

From Newton's meaſures of the thickneſſes reflecting the different colours, the breadth and duration of their reſpective undulations may be very accurately determined; although it is not improbable, that when the glaſſes approach very near, the atmoſphere of ether may produce ſome little irregularity. The whole viſible ſpectrum appears to be compriſed within the ratio of three to five, or a major ſixth in muſic; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 8, 7, and 6; ſo that the interval from red to blue is a fourth. The abſolute frequency expreſſed in numbers is too great to be diſtinctly conceived, but it may be better imagined by a compariſon with ſound.

and compared with thoſe of ſound.

If a chord ſounding the tenor $\overset{\frown}{c}$, could be continually biſected 40 times, and ſhould then vibrate, it would afford a yellow green light: this bring denoted by $\overset{41}{c}$, the extreme red would be $\overset{40}{a}$, and the blue $\overset{41}{d}$. The abſolute length and frequency of each vibration is expreſſed in the table; ſuppoſing light to travel in 8¼ minutes 500,000,000,000 feet.

Colours.

Table of colorific undulations of the ether.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme -	.0000266	37640	463 millions of millions.
Red - -	.0000256	39180	482
Intermediate	.0000246	40720	501
Orange - -	.0000240	41610	512
Intermediate	.0000235	42510	523
Yellow - -	.0000227	44000	542
Intermediate	.0000219	45600	561 $(=2^{45}$ nearly)
Green - -	.0000211	47460	584
Intermediate	.0000203	49320	607
Blue - -	.0000196	51110	629
Intermediate	.0000189	52910	652
Indigo - -	.0000185	54070	665
Intermediate	.0000181	55240	680
Violet - -	.0000174	57490	707
Extreme -	.0000167	59750	735

Scholium. It was not till I had fatisfied myfelf refpecting all thefe phenomena, that I found in Hooke's Micrographia, a paffage which might have led me earlier to a fimilar conclufion. "It is moft evident that the reflection from the under or further fide of the body, is the principal caufe of the production of thefe colours.—Let the ray fall obliquely on the thin plate, part therefore is reflected back by the firft fuperficies, —part refracted to the fecond furface,—whence it is reflected and refracted again.—So that, after two refractions and one reflection, there is propagated a kind of fainter ray—," and, "by reafon of the time fpent in paffing and repaffing,—this fainter pulfe comes behind the" former reflected "pulfe; fo that hereby, (the furfaces being fo near together that the eye cannot difcriminate them from one,) this confufed or duplicated pulfe, whofe ftrongeft part precedes, and whofe weakeft follows, does produce on the retina, the fenfation of a yellow. If thefe furfaces are further removed afunder, the weaker pulfe may become coincident with the" reflection of the fecond," or next following pulfe, from the firft furface, "and lagg behind that alfo, and be coincident with the third, fourth, fifth, fixth, feventh, or eighth—; fo that, if there be a thin tranfparent body, that from the greateft thinnefs requifite,

Quotation from Robert Hooke exhibiting a fimilar doctrine,

fite to produce colours, does by degrees grow to the greateſt thickneſs, the colours ſhall be ſo often repeated, as the weaker pulſe does loſe paces with its primary or firſt pulſe, and is coincident with a" ſubſequent " pulſe. And this, as it is coincident, or follows from the firſt hypotheſis I took of colours, ſo upon experiment have I found it in multitudes of inſtances that ſeem to prove it." (P. 65—67.) This was printed about ſeven years before any of Newton's experiments were made. We are informed by Newton, that Hooke was afterwards diſpoſed to adopt his " ſuggeſtion" of the nature of colours; and yet it does not appear that Hooke ever applied that improvement to his explanation of theſe phenomena, or inquired into the neceſſary conſequence of a change of obliquity, upon his original ſuppoſition, otherwiſe he could not but have diſcovered a ſtriking coincidence with the meaſures laid down by Newton from experiment. All former attempts to explain the colours of thin plates, have either proceeded on ſuppoſitions which, like Newton's, would lead us to expect the greateſt irregularities in the direction of the refracted rays; or, like Mr. Michell's, would require ſuch effects from the change of the angle of incidence, as are contrary to the effects obſerved; or they are equally deficient with reſpect to both theſe circumſtances, and are inconſiſtent with the moſt moderate attention to the principal phenomena.

which was printed ſeven years before Newton made his experiments.

COROLLARY 3. *Of the Colours of thick Plates.*

When a beam of light paſſes through a refracting ſurface, eſpecially if imperfectly poliſhed, a portion of it is irregularly ſcattered, and makes the ſurface viſible in all directions, but moſt conſpicuouſly in directions not far diſtant from that of the light itſelf: and, if a reflecting ſurface be placed parallel to the refracting ſurface, this ſcattered light, as well as the principal beam, will be reflected, and there will alſo be a new diſſipation of light, at the return of the beam through the refracting ſurface. Theſe two portions of ſcattered light will coincide in direction; and, if the ſurfaces be of ſuch a form as to collect the ſimilar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the ſcattered light between the two ſurfaces; of courſe, wherever the inclination of the ſcattered light is equal to that of the beam, although in diffe-

Colours of thick plates explained from the greater length of ſuch rays of ſcattered light (compared with the principal ray) as paſs through the firſt ſurface, and are reflected from the ſecond with a different inclination.

rent planes, the interval will vanifh, and all the undulations will confpire. At other inclinations, the interval will be the difference of the fecants from the fecant of the inclination or angle of refraction of the principal beam. From thefe caufes, all the colours of concave mirrors obferved by Newton and others are neceffary confequences: and it appears that their production, though fomewhat fimilar, is by no means, as Newton imagined, identical with the production of thofe of thin plates.

COROLLARY 4. *Of Blacknefs.*

In the three preceding corollaries, we have confidered the refracting and reflecting fubftances as limited by a mathematical furface; but this is perhaps never phyfically true. The ethereal atmofpheres may extend on each fide the furface as far as the breadth of one or more undulations; and, if they be fuppofed to vary equally in denfity at every part, the partial reflections from each of the infinite number of furfaces, where the denfity changes, will very much interfere with each other, and deftroy a confiderable portion of the reflected light, fo that the fubftance may become pofitively black; and this effect may take place in a greater or lefs degree, as the denfity of the ethereal atmofphere varies more or lefs equably; and, in fome cafes, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. Bouguer has obferved a confiderable lofs of light, and in fome inftances a tinge of colour, in total reflections at the furface of a rarer medium.

Blacknefs produced by innumerable reflections at the confine of two furfaces where the ether gradually varies in denfity: the effect of which is, that the undulations confound and deftroy each other, and confequently the fenfation of light.

COROLLARY 5. *Of Colours by Inflection.*

Whatever may be the caufe of the inflection of light paffing through a fmall aperture, the light neareft its centre muft be the leaft diverted, and the neareft to its fides the moft; another portion of light falling very obliquely on the margin of the aperture, will be copioufly reflected in various directions; fome of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous route, will fo interfere with it, as to caufe an appearance of colours. The length of the two tracks will differ the lefs, as the direction of the reflected light has been lefs changed by its reflection, that is, in the light paffing neareft to

Colours by inflection explained by the different lengths of the bended rays occafioning alternations in the compound undulations as in Corollary 1.

to the margin; fo that the blues will appear in the light neareft the fhadow. The effect will be increafed and modified, when the reflected light falls within the influence of the oppofite edge, fo as to interfere with the light fimply inflected by that alfo.

More particular examination of the confequences.

But, in order to examine the confequences more minutely, it will be convenient to fuppofe the inflection caufed by an ethereal atmofphere, of a denfity varying as a given power of the diftance from a centre, as in the eighth propofition of the laft Bakerian Lecture. (Phil. Tranf. for 1801, p. 83.) Putting $r = 3$, and $x = \frac{1}{2}$, I have conftructed a diagram, (Fig. 4,) which fhows, by the two pair of curves, the relative pofition of the reflected and unreflected portions of any one undulation at two fucceffive times, and alfo, by fhaded lines drawn acrofs, the parts where the intervals of retardation are in arithmetical progreffion, and where fimilar colours will be exhibited at different diftances from the inflecting fubftance. The refult fully agrees with the obfervations of Newton's third book, and with thofe of later writers. But I do not confider it as quite certain, until further experiments have been made on the inflecting power of different fubftances, that Dr. Hooke's explanation of inflection, by the tendency of light to diverge, may not have fome pretenfions to truth. I am forry to be obliged to recall here the affent which, at firft fight, I was induced to give to a fuppofed improvement of a late author. (Phil. Tranf. for 1800, p. 128.)

Scholium. In the conftruction of the diagram, it becomes neceffary to find the time fpent by each ray in its paffage. Since the velocity was denoted by $x^{-\frac{1}{r}}$, on the fuppofition of a projectile, it will be as $x^{\frac{1}{r}}$ on the contrary fuppofition, (Phil. Tranf. for 1801, p. 27. *Schol.* 2. Prop. I.) and the fluxion of the diftance defcribed being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the time will be $\frac{x^{-\frac{1}{r}}\dot{x}}{\sqrt{1-yy}}$, or $\frac{rs}{1-r}.\ \frac{\dot{y}}{yy.\sqrt{1-yy}}$, of which the fluent is $\frac{r}{1-r}.\ \frac{s}{y}.\ \sqrt{1-yy}$. Therefore, with the radius $x^{1-\frac{1}{r}}$, defcribe

defcribe a circle concentric with the furfaces of the inflecting atmofphere, then the angle defcribed by the ray during its paffage through the atmofphere, will always be to the angle fubtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-i$; and the time fpent in this paffage, will be in the fame ratio to the time that would have been fpent in defcribing this intercepted portion with the initial velocity. For y, being equal to $s x^{\frac{1}{r}-1}$, is the fine of the inclination of the incident ray to the radius, where it meets this circle; therefore by the propofition quoted, the angle defcribed is in a given ratio to the angle at the centre, which is the difference of the inclinations. Making $x^{1-\frac{1}{r}}$ or $\frac{s}{y}$ radius, the fine, inftead of y, becomes s, and the cofine $\sqrt{\frac{ss}{yy}-ss}$, or $\frac{s}{y}\sqrt{1-yy}$, and, when $y=ss$, $\sqrt{1-ss}$; therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See alfo Young's Syllabus, Art. 372.)

PROPOSITION IX.

Radiant Light confifts in Undulations of the luminiferous Ether.

Conclufion. Radiant light confifts in undulations of the ether.

This propofition is the general conclufion from all the preceding; and it is conceived that they confpire to prove it in as fatisfactory a manner as can poffibly be expected from the nature of the fubject. It is clearly granted by Newton, that there are undulations, yet he denies that they conftitute light; but it is fhown in the three firft Corollaries of the laft Propofition, that all cafes of the increafe or diminution of light are referable to an increafe or diminution of fuch undulations, and that all the affections to which the undulations would be liable, are diftinctly vifible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will ferve to obviate fome objections which may be raifed againft this theory.

Remarks. Newton without giving a reafon rejected the law of Huygens in explaining the phenomena of Iceland cryftal.

1. Newton has advanced the fingular refraction of the Iceland cryftal, as an argument that the particles of light muft be projected corpufcles; fince he thinks it probable that the different fides of thefe particles muft be differently attracted by the cryftal, and fince Haygens has confeffed his inability to account

account in a fatisfactory manner for all the phenomena. But, contrarily to what might have been expected from Newton's ufual accuracy and candour, he has laid down a new law for the refraction, without giving a reafon for rejecting that of Huygens, which Mr. Hauy has found to be more accurate than Newton's; and, without attempting to deduce from his own fyftem any explanation of the more univerfal and ftriking effects of doubling fpars, he has omitted to obferve that Huygens's moft elegant and ingenious theory perfectly accords with thefe general effects, in all particulars, and of courfe derives from them additional pretenfions to truth: this he omits, in order to point out a difficulty, for which only a verbal folution can be found in his own theory, and which will probably long remain unexplained by any other.

Michell's experiments on the momentum of light explained, without fuppofing any projection.

2. Mr. Michell has made fome experiments, which appear to fhow that the rays of light have an actual momentum, by means of which a motion is produced when they fall on a thin plate of copper delicately fufpended. (Prieftley's Optics.) But, taking for granted the exact perpendicularity of the plate, and the abfence of any afcending current of air, yet fince, in every fuch experiment, a greater quantity of heat muft be communicated to the air at the furface on which the light falls than at the oppofite furface, the excefs of expanfion muft neceffarily produce an excefs of preffure on the firft furface, and a very perceptible receffion of the plate in the direction of the light. Mr. Bennet has repeated the experiment, with a much more fenfible apparatus, and alfo in the abfence of air; and very juftly infers from its total failure, an argument in favour of the undulatory fyftem of light. (Phil. Tranf. for 1792, p. 87.) For, granting the utmoft imaginable fubtility of the corpufcles of light, their effects might naturally be expected to bear fome proportion to the effects of the much lefs rapid motions of the electrical fluid, which are fo very eafily perceptible, even in their weakeft ftates.

They did not fucceed with Bennet.

Latent light and heat not inconfiftent with the doctrine of vibrations.

3. There are fome phenomena of the light of folar phofphori, which at firft fight might feem to favour the corpufcular fyftem; for inftance, its remaining many months as if in a latent ftate, and its fubfequent re-emiffion by the action of heat. But, on further confideration, there is no difficulty in fuppofing the particles of the phofphori which have been made to vibrate by the action of light, to have this action abruptly fufpended

fufpended by the intervention of cold, whether as contracting the bulk of the fubftance or otherwife; and again, after the reftraint is removed, to proceed in their motion, as a fpring would do which had been held faft for a time in an intermediate ftage of its vibration; nor is it impoffible that heat itfelf may, in fome circumftances, become in a fimilar manner latent. (Nicholfon's Journal, Vol. II. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at prefent, it feems highly probable that light differs from heat only in the frequency of its undulations or vibrations; thofe undulations which are within certain limits, with refpect to frequency, being capable of affecting the optic nerve, and conftituting light; and thofe which are flower, and probably ftronger, conftituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or tranfient ftate; vibratory light being the minute motion of ignited bodies, or of folar phofphori, and undulatory or radiant light the motion of the ethereal medium excited by thefe vibrations; vibratory heat being a motion to which all material fubftances are liable, and which is more or lefs permanent; and undulatory heat that motion of the fame ethereal medium, which has been fhown by Mr. King, (Morfels of Criticifm, 1786, p. 99,) and M. Pictet, (*Effais de Phyfique*, 1790, alfo in Sauffure's Voyage dans les Alpes, 1786.) to be as capable of reflection as light, and by Dr. Herfchel to be capable of feparate refraction. (Phil. Tranf. for 1800, p. 284,) How much more readily heat is communicated by the free accefs of colder fubftances, than either by radiation or by tranfmiffion through a quiefcent medium, has been fhown by the valuable experiments of Count Rumford. It is eafy to conceive that fome fubftances, permeable to light, may be unfit for the tranfmiffion of heat, in the fame manner as particular fubftances may tranfmit fome kinds of light, while they are opaque with refpect to others.

Light and heat differ only in the frequency of undulation or vibration,

capable in both cafes of the fame modifications.

On the whole it appears, that the few optical phenomena which admit of explanation by the corpufcular fyftem, are equally confiftent with this theory; that many others, which have long been known, but never underftood, become by thefe means perfectly intelligible; and that feveral new facts are found to be thus only reducible to a perfect analogy with other facts, and to the fimple principles of the undulatory fyftem.

This theory explains all that is folved by the corpufcular fyftem, and much more.

fystem. It is presumed, that henceforth the fecond and third books of Newton's Optics will be confidered as more fully underftood than the firft has hitherto been; but, if it fhould appear to impartial judges, that additional evidence is wanting for the eftablifhment of the theory, it will be eafy to enter more minutely into the details of various experiments, and to fhow the infuperable difficulties attending the Newtonian doctrines, which, without neceffity, it would be tedious and invidious to enumerate. The merits of their author in natural philofophy, are great beyond all conteft or comparifon; his optical difcovery of the compofition of white light, would alone have immortalifed his name; and the very arguments which tend to overthrow his fyftem, give the ftrongeft proofs of the admirable accuracy of his experiments.

Experiment in which the corpufcular theory ought to exhibit diftortion.

Sufficient and decifive as thefe arguments appear, it cannot be fuperfluous to feek for further confirmation; which may with confiderable confidence be expected, from an experiment very ingenioufly fuggefted by Profeffor Robifon, on the refraction of the light returning to us from the oppofite margins of Saturn's ring; for, on the corpufcular theory, the ring muft be confiderably diftorted when viewed through an achromatic prifm: a fimilar diftortion ought alfo to be obferved in the difc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from thefe planets, there can fcarcely be any remaining hope to explain the affections of light, by a comparifon with the motions of projectiles.

IV.

An Analyfis of a Mineral Subftance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, *Efq.*

(Concluded from Page 138.)

H.

The white precipitate was little affected by diftillation of the fulphuric and nitric acids.

THE white precipitate, when diftilled with four parts of fulphur, remained pulverulent, and, from white, was only changed to a pale afh colour.

Nitric acid was digefted on the powder, and, being heated, afforded fome nitrous gas; after this, the powder became white, and in every refpect recovered its original properties.

Before

I.

Before I conclude this ſection, I muſt obſerve, that when the olive-green precipitates, obtained by pruſſiate of potaſh, were digeſted in an alkaline lixivium, they were decompoſed; for the alkali combined with the pruſſic acid, and with a ſmall part of the white matter; but the greater part of the latter remained undiſſolved, in the ſame white flocculent ſtate which was noticed when the alkaline combinations were mentioned.

The precipitates by pruſſ. pot. were decompoſed by humid alkali, which took up the pr. acid and a little of the white matter.

The orange-coloured precipitates, formed by tincture of galls, were alſo decompoſed when digeſted in boiling nitric acid; and the white matter was recovered in its original ſtate.

§ III. REMARKS.

The preceding experiments ſhew, that the ore which has been analyſed, conſiſts of iron combined with an unknown ſubſtance, and that the latter conſtitutes more than three-fourths of the whole. This ſubſtance is proved to be of a metallic nature, by the coloured precipitates which it forms with pruſſiate of potaſh, and with tincture of galls; by the effects which zinc produces, when immerſed in the acid ſolutions; and by the colour which it communicates to phoſphate of ammonia, or rather to concrete phoſphoric acid, when melted with it.

Obſ. The ore is iron, with three parts of the white matter. The latter is metallic; for it is precip. by pruſſiate, and by galls, and by zinc, and it colours phoſ. acid by fuſion.

Moreover, from the experiments made with the blow-pipe, it ſeems to be one of thoſe metallic ſubſtances which retain oxigen with great obſtinacy, and are therefore of difficult reduction.

It is of difficult reduction,

It is an acidifiable metal; for the oxide reddens litmus paper, expels carbonic acid, and forms combinations with the fixed alkalis. But it is very different from the acidifiable metals which have of late been diſcovered; for,

and acidifiable.

1. It remains white when digeſted with nitric acid.

2. It is ſoluble in the ſulphuric and muriatic acids, and forms colourleſs ſolutions, from which it may be precipitated, in the ſtate of a white flocculent oxide, by zinc, by the fixed alkalies, and by ammonia. Water alſo precipitates it from the ſulphuric ſolution, in the ſtate of a ſulphate.

Differs from all other the acidifiable metals, in the properties here enumerated.

3. Pruſſiate of potaſh produces a copious and beautiful olive-green precipitate.

other ſubſtances, which are now conſidered as ſimple, primitive, and diſtinct bodies, will be found to be compounds. Yet I only entertain and ſtate this opinion as a probability; for, until an advanced ſtate of chemical knowledge ſhall enable us to compoſe, or at leaſt to decompoſe, theſe bodies, each muſt be claſſed and denominated as a ſubſtance *ſui generis*. Conſidering, therefore, that the metal which has been examined is ſo very different from thoſe hitherto diſcovered, it appeared proper that it ſhould be diſtinguiſhed by a peculiar name; and, having conſulted with ſeveral of the eminent and ingenious chemiſts of this country, I have been induced to give it the name of Columbium.

POSTSCRIPT.

It appears proper to mention ſome unſucceſsful attempts which I have lately made to reduce the white oxide.

Reduction. The white powder was only blackened by the ſtrong heat with charcoal.

Fifty grains were put into a crucible coated with charcoal; and being covered with the ſame, the crucible was cloſely luted, and was expoſed to a ſtrong heat, in a ſmall wind furnace, during about one hour and an half. When the crucible was broken, the oxide was found in a pulverulent ſtate; and, from white, was become perfectly black.

The phoſphuret.

In order to form a phoſphuret, ſome phoſphoric acid was poured upon a portion of the white oxide; and, being evaporated to dryneſs, the whole was put into a crucible coated with charcoal, as above deſcribed. The crucible was then placed in a forge belonging to Mr. Chenevix, and a ſtrong heat was kept up for half an hour.

The incloſed matter was ſpongy, and of a dark brown; it in ſome meaſure reſembled phoſphuret of titanium.

After this we wiſhed to try the effect of a ſtill greater heat; but in this experiment the crucible was melted.

The above experiments ſhew, that the white oxide, like ſeveral other metallic ſubſtances, may be deoxidated to a certain degree, without much difficulty, but that the complete reduction of it is ſtill far from being eaſily effected.

V. *On*

V.

On the Effect of Sound upon the Barometer. By Sir HENRY C. ENGLEFIELD, *Bart. F. R. S.* (*From the Journals of the Royal Institution, No. 9.*)

DURING the time I ſpent at Bruſſels in the year 1773 and 1774, it occurred to me, that the effect of ſound on the barometer had not, to my knowledge, been attended to; and that it was by no means certain, whether that inſtrument was capable of being ſenſibly affected by thoſe elaſtic vibrations cauſed in the atmoſphere, by the percuſſion of a ſonorous body. I thought the idea worthy of being purſued, and the means of making ſatisfactory experiments were moſt opportunely in my power. Whether the barometer be affected by ſonorous undulation.

The ſound of a very large bell appeared to me the moſt powerful, and at the ſame time to be approached with the greateſt ſecurity and eaſe to the obſerver. The exploſion of artillery, beſides the very diſagreeable ſmoke and danger of the recoil, might be objected to, on account of the ſudden production of elaſtic and heated vapour, which might, independent of the ſound, inſtantaneouſly alter the ſtate of the atmoſphere, and thereby lead the obſerver into very great and unavoidable errors. The bell preferred for experiments.

Every one who has been in the Low Countries muſt know, that very large bells, and immenſe numbers of them, are the pride of their churches; and that they are rung quite out, not tolled, on every great feſtival. The great bell of the collegiate church of St. Gudula, at Bruſſels, weighs, as I was told, ſixteen thouſand pounds, and on this I determined to found my experiment. Large bells in the Netherlands.

Two objections only could be made to the reſult of this experiment, the one, that the motion of the bell might cauſe a vibration in the walls of the building, which would hinder the placing the barometer in a ſtate of repoſe; the other, that the ſwinging ſo large a maſs with a conſiderable degree of velocity, might of itſelf agitate the air ſo as to cauſe vibrations in the mercury totally independent of ſound. Whether theſe would produce irregularity by agitating the building or the air.

The ſtrength of the walls of the ſteeple, and the manner of hanging the bell, which was contained in a frame of timber, founded Obſervations.

founded on a ftrong vault, and totally independent of the wall of the fteeple, might alone have anfwered the firft of thefe objections; but happily a moft complete and fatisfactory anfwer to both of them, was furnifhed by the manner in which the bell was rung.

Preparation for experiment.

As the bell was to ring out full in an inftant, at a fignal given from below, it is neceffary to have it in motion fome time beforehand; and during that time, the clapper is fixed to one fide by a ftrong ftick croffing the mouth of the bell, which, at the fignal, is pulled out by the hand of a perfon placed for that purpofe. If then, our barometer fhewed no variation during all this time, we were abfolutely certain that whatever motion was perceived afterwards, was wholly owing to the found.

Mr. Pigott, who was then at Bruffels, was kind enough to lend me one of his barometers, made by Ramfden, and his fon made the following obfervations jointly with myfelf:

Narrative.

At two o'clock in the afternoon of the 1ft of November 1773, we went into the northweft tower of St. Gudula's church, and having fixed the barometer firmly in the opening of a window, not above feven feet from the bottom of the bell, we waited quietly for its ringing.

The height of the mercury before the bell began to fwing, as obferved by Mr. Pigott, was 29.478 inches. The bell being in full fwing, no alteration whatever was perceptible.

The mercury was elevated by the found of a large bell.

The inftant that the clapper was loofed the mercury leaped up, and continued that fort of fpringing motion, at every ftroke of the clapper, during the whole time of the ringing of the bell. Thefe were our obfervations:

During the ringing of the bell, Mr. P. -	29.469
During the ringing, by myfelf.	
Higheft - - - - - -	29.480
Loweft - - - - - -	29.474
Higheft - - - - - -	29.482
Loweft - - - - - -	29.472

Thefe obfervations were made with the greateft attention; and, confidering their delicacy and the difficulty of obferving, agree very nearly. They appear to give from 6 to 10 thoufandths of an inch for the effect of this found on the barometer. It is to be obferved, that Mr. Pigott, in general, eftimated the

the height of the mercury about 5 thoufandths lower than myfelf, which brings our obfervations to a very near agreement. The following obfervations prove this:

On the top of the tower, Mr. P. - -	29.424
Ditto, by me - - - - -	29.430
At the foot of the tower, Mr. P. - -	29.639
Ditto, by me - - - - -	29.642
In the court of the Englifh Nuns, by Mr. P.	29.676
Ditto, by me - - - - -	29.682

And I fhould think that the difference of eyes may frequently caufe fuch a variation among different obfervers; at leaft in delicate obfervations, it will be always prudent to make the experiment.

NOTE BY DR. YOUNG.

Obfervations by Dr. Young.

THESE obfervations appear to agree too well with each other, to allow us to doubt of their accuracy. It therefore becomes neceffary to inquire into the caufes of the different heights of the barometer. It is indeed barely poffible, that a fudden ftroke of the clapper on the bell might produce a greater agitation of the building than the preceding alternate motion of the bell itfelf: but this explanation cannot be called fatisfactory. It is certain, that there was neither more nor lefs air in the tower while the bell was founding, than while it was filent; the mean denfity of the air could therefore not have been changed; and if the alternate motions of the particles of air which conftitute found, had taken place by equal degrees, and with equal velocities, in each oppofite direction, there is no reafon to fuppofe that the increafe of preffure on the furface of the mercury, at one inftant, could have tended to raife it, more than the decreafe of preffure, in the oppofite ftate of the undulation, would have depreffed it. But the fame confequence does not follow, if we conceive the motion of the air, in advancing, to be more rapid, but of fhorter continuance, than its retrogade motion. For if the wind blew for one hour with a velocity of 4, and the fame air returned, in the courfe of two hours, with a velocity of 2, an obftacle upon which it had acted in both directions would not be found in its original place; for the action of the wind upon an obftacle is as the fquare

The mean denfity of the air was not changed.

If the motion of advance were more rapid than of return in the undulations, it would have the effect of a preffure.

ſquare of the velocity, and the time would not compenſate for the difference of force. It is therefore eaſy to ſuppoſe, that the law of the bell's vibration was in this experiment ſuch, that the air advanced towards the barometer with a greater velocity than it receded, although for a ſhorter time; and that hence the whole effect was the ſame as if the mean preſſure of the air had been increaſed. Such a law might eaſily reſult from a combination of a more regular principal vibration with one or more ſubordinate ones, in different relations; and ſimilar caſes may ſometimes be obſerved in the vibrations of chords. Here we find a ſlight degree of repulſion, in conſequence of the undulations of an elaſtic medium. Dr. Hooke attempted to explain the phenomena of attraction by means of ſimilar undulations of an ether, which he ſuppoſed to be the medium ſerving for the communication of heat; but it muſt be confeſſed, that the conjecture has little appearance of probability.

This law might eaſily reſult from ſubordinate vibrations, combining with the principal.

Slight repulſions from this cauſe in elaſtic mediums.

Whether gravity be ſo cauſed.

VI.

On the Expanſion of Carbonated Hidrogen by Electricity. From a Correſpondent.

To Mr. NICHOLSON.

SIR,

Expanſion of carb. hidrogen by electricity not yet explained.

I VENTURE to trouble you with a few remarks on a phenomenon in chemiſtry, the expanſion of carbonated hidrogen gas on the electric ſpark being paſſed through it. None of the explanations of this circumſtance which I have met with, appear to me to be ſatisfactory; and I have ſtated a few objections which occurred to me on conſidering them. If you think them of ſufficient weight to fill a corner of your ably-conducted Journal, ſome correſpondent of greater abilities may perhaps give a new explanation of the fact. But if you think them beneath your notice, and unworthy of a place among the excellent communications with which your numbers are filled, I have only to beg that you will pardon the preſumption of a very young chemiſt, who, by this firſt attempt, has perhaps only expoſed his own ignorance.

I am, Sir,

Your's with much reſpect,

Edin. June 8, 1802. G. H.

On

On the Expanſion of carbonated Hidrogen Gas.

This fact was firſt obſerved by Dr. Auſtin. On paſſing the electric ſpark through a quantity of carbonated hidrogen gas, he found that the gas was permanently dilated to more than twice its original bulk. He concluded that this remarkable expanſion could only be owing to the evolution of hidrogen gas. Upon burning the air thus expanded in oxigen gas, he found that it required more oxigen for its combuſtion than an equal quantity of carbonated hidrogen gas, which had not been expanded by the electric ſpark. *An addition therefore had been made to the combuſtible matter;* for the quantity of oxigen neceſſary to complete the combuſtion of any body, is always proportional to the quantity of that body. He concluded from theſe experiments, that he had decompoſed the carbon which had been diſſolved in the hidrogen, and that carbon was compoſed of hidrogen and azote, ſome of which was always found in the veſſel after the combuſtion.

Dr. Auſtin firſt obſerved that carbon. hidrogen is expanded to double by electricity.

His theory that carbon is hidrogen and azote.

If Dr. Auſtin had more attentively conſidered the circumſtances of theſe experiments, he would probably been prevented from drawing this concluſion. *The quantity of combuſtible matter had been increaſed.* Now, if the expanſion of the carbonated hidrogen gas was owing merely to the decompoſition of carbon, no ſuch increaſe ought to have taken place, but rather the contrary; for the carbon, which was itſelf a combuſtible ſubſtance, was reſolved into two ingredients, hidrogen and azote, only the firſt of which burnt on the addition of oxigen, and the application of heat. And beſides, if the carbon had been reſolved into hidrogen and azote, the product of the combuſtion could only have been a greater quantity of water, with a reſiduum of azote; for the hidrogen evolved by the dilatation of the carbonated hidrogen gas, combining with the additional oxigen, muſt have formed an additional quantity of water. But it is a fact, which the Doctor does not ſeem to have attended to, that beſides water, *carbonic acid gas* is produced from the combuſtion of the expanded carbonated hidrogen. Whence, then, comes the carbonic acid gas?

Objections. Hidrogen and azote are not more combuſtible than carbon: and the product is not mere water and azote, but alſo carbonic acid gas.

Mr. Henry *, who repeated Dr. Auſtin's experiments with great accuracy, found that he was correct with regard to the

Mr. Henry found that the expanſion is

* Philoſ. Tranſact. 1797.

4 increaſe

produced by hidrogen from decompofed water.

increafe of combuftible matter. He alfo found, that the expanfion could not be carried beyond a certain point, about twice the original bulk of the gas. Upon burning feparately by means of oxigen, two equal portions of carbonated hidrogen gas, one of which had been expended by electricity to twice its original bulk, the other not, he found that each of them produced precifely the fame quantity of carbonic acid gas; a proof, that the carbon in both remained the fame, and that the hidrogen could not have been produced from it. He concluded therefore, that the evolution of the hidrogen is produced from the decompofition of the water with which hidrogen is always more or lefs impregnated; to prove which, he took hidrogen gas, from which he expelled as much water as poffible, and found that the electric fpark produced a very fmall dilatation; but on admitting a drop or two of water, the expanfion went on as ufual. It is eafily feen how this decompofition is effected. Carbon at a high temperature has a greater affinity for oxigen than hidrogen gas; when the temperature is raifed, therefore, by the electric fpark, the carbon unites with the oxigen of the water, forming carbonic acid gas, and the hidrogen is evolved. As to the azote, it muft have been produced from the admiffion of atmofpheric air into the procefs.

But this difcovery does not account for the greater quantity of oxigen required to burn the expanded gas.

Although this theory of Mr. Henry feems to prove the production of hidrogen from the decompofition of the water contained in the gas; ftill, however, it is no more able to account for the increafe of combuftible matter than that of Dr. Auftin. When the gas is dilated, hidrogen and oxigen are evolved from the water. The hidrogen goes to increafe the bulk of the gas, and the oxigen unites with the carbon. Expanded carbonated hidrogen gas, then, contains hidrogen, carbon, and carbonic acid gas. In combuftion, the evolved hidrogen, in order to form water, requires a quantity of additional oxigen, precifely the fame as that with which it was combined before decompofition of the water: and on the other hand, the carbon requires precifely the fame quantity lefs, as part of it is already rendered incombuftible, by being combined with the oxigen of the water. Therefore, according to Mr. Henry's theory, a quantity of expanded carbonated hidrogen gas requires the fame quantity of oxigen for its combuftion, as an equal quantity that has not been expanded: and

and confequently they both contain the fame quantity of combuftible matter; for in proportion as the combuftible matter is increafed by the formation of hidrogen, in the fame proportion it is diminifhed by the formation of carbonic acid gas. But this is obferved to be contrary to fact; for carbonated hidrogen gas when expanded does actually contain more combuftible matter than before its expanfion. It would appear then, that a fatisfactory explanation of this increafe of combuftible matter has not yet been given.

G. H.

VII.

A new Procefs for claying Sugars, propofed by CIT. HAPEL-LACHENAIE, *Chief Apothecary of the Military Hofpitals of Guadaloupe, to the Agents of the Confuls of the French Republic in the Windward Iflands* *.

BEING called upon by the fcientific miffion confided to me by the Government, to employ myfelf in every kind of refearch which may prove interefting to the cultivation and productions of this colony, I have thought it my duty to endeavour to difcover a fimple, eafy, and cheap method of difpenfing with the pottery which is ufed in the fabrication of clayed fugars. Introduction.

There was even an urgent neceffity that I fhould direct my attention to thefe objects, becaufe moft of the proprietors of thefe colonies, being at this moment in want of the forms and pots hitherto confidered as indifpenfibly neceffary for the claying fugars, are obliged to wait till the fmall number of pots they poffefs fhall be cleared of the fugar they contain before they can fabricate more. Want of Pans for claying fugars in St. Domingo,

The delays occafioned by this inconvenience are the more prejudicial to the inhabitants in general, becaufe they are obliged, on this account, to defer cutting their canes when at the moft advantageous time of their growth; and as they cannot perform this at the moft favourable feafon, their loffes are incalculable by fuffering the canes to grow old, and finifhing their obvioufly productive of great lofs.

* From the Annales de Chimie, XL. 73.

their gathering in the season when they afford very little sugar, in comparison with what they would have yielded if they had been cut in time.

While this retardation diminishes the immediate product, it has an influence on the vigour of the succeeding shoots, as I have observed, and renders their advancement less speedy. And by this means it exposes the farmer of national plantations to fail in payment of the terms of their location.

Experiments for a new process,

I shall not dwell upon the various experiments I made before my success was complete, but shall confine myself to those means which have best succeeded with me, and to which I have confined myself in the subsequent practice in my sugar-work.

for disposing the sugar to be clayed in large vessels.

This process consists in disposing the sugar in receptacles of 12,937 cubic feet, each containing 26 ordinary forms, and to clay it in these same receptacles, of which I shall proceed to give a description:

First used by Citizens Boucherie at Paris,

The priority of the method which I propose belongs to Cits. Boucherie, who first adopted it in their refinery of Bercy near Paris. They were, as far as I know, the first who constructed vessels to clay the raw sugars they received from our colonies previous to their refining them. But though this invention is undoubtedly theirs, I also have a claim as to the degree of improvement which their vessel did not possess, but which I have given them by rendering them more convenient, advantageous, and economical *.

and improved by the Author.

Description of the apparatus of Citizens Boucherie.

The vessels of Citizens Boucherie were, as nearly as I can recollect, about 15 or 18 inches deep, and five feet wide. They were square, and the bottom of each was perforated with a great number of small holes for the discharge of the syrup, which fell into a second vessel less deep, but of the same dimensions as the others as to their width. This second vessel was lined with thin metallic plates, of an alloy invented by Mr. Hiskerdeau, a Spanish chemist. The upper vessels

* I attended their operations in the year 1784, before I came to this colony. I had also the advantage at that time of giving several lectures in their establishment to some cultivators of St. Domingo and other sugar islands, to facilitate their knowledge of the doctrine and processes of that interesting manufacture which they frequently visited.

were

were ſupported on their edges by a level and firm framing, the inferior veſſels ſtood on the floor.

The upper veſſels were filled with raw ſugar intended to be clayed, which was firſt well divided or crumbled in order to its equal diſtribution. This was levelled and compreſſed as equally as poſſible, to form what we commonly call, the bottom *(les fonds)*. Upon this maſs the diluted and prepared earth, properly adapted to the operation, was very carefully poured. Charged with raw ſugar.

Though this proceſs may appear very ſimple at firſt ſight, it nevertheleſs preſents difficulties which could ſcarcely be overcome in that manufactory, and which would be almoſt impoſſible to ſurmount in ours, where the men employed in this buſineſs have neither the underſtanding nor the ſkill of thoſe of Citizens Boucherie; and it was not till they had practiſed for ſome time that they became able to perform it without difficulty. This ſimple proceſs is liable to objections.

The firſt operation in which a failure may be made for want of ſkill, is that of levelling with a trowel an elaſtic ſurface of 25 ſquare feet. The ſecond is to compreſs equally that ſurface in order to give ſolidity to the bottom, that the diluted earth may extend to the ſame thickneſs on all ſides, and find that ſurface ſo cloſe as to admit the penetration of the water only. It is difficult to level and preſs the ſugar in the large veſſels.

All thoſe who are engaged in the manufacture of ſugar muſt be aware, that if any inequality exiſts in the levelling of the bottom, the water which gradually leaves the earth naturally flows to the loweſt place, where, if the compreſſion be not every where the ſame, the water inſinuates itſelf into the moſt porous part; and in either of theſe caſes, this fluid being conducted with the earth towards a ſingle point, is collected in ſufficient abundance to diſſolve the ſugar at that place, and form what is called a fox (*un renard.*) When this happens the operation of claying fails, for it is known that the portion of earth retained at the ſurface acts little upon the reſt of the maſs of ſugar. This inconvenience, which occaſions a great loſs in the product, is alſo found to change its quality, which is worſe on this account. Miſchievous conſequences of bad levelling or preſſing. The water flows to the loweſt place and diſſolves the ſugar; while the reſt is not full purified.

In the conſtruction of the caſes or veſſels of Citizens Boucherie, which cannot be removed, other inconveniencies are found which do not exiſt in mine. One of theſe is the difficulty The ſugar is not eaſily taken out.

culty of taking out the fugar after its drainage. A fecond is, that the holes in their bottom are too fmall, which renders them liable to be obftructed, and the procefs to be greatly retarded.

(To be concluded in our next.)

VIII.

Defcription of the Cryftalline Forms of the Anhydrous Sulphate of Lime, with fome Obfervations on this Subftance. By M. Le Comte de Bournon, *Member of the Royal and Linnæan Societies of London. Tranflated from the Original, communicated by the Author* *.

Sulphate of lime containing no water is a new object in mineralogy.

GYPSUM, or the fulphate of lime deprived of water, is a new object in mineralogy: fcarcely has the fcience as yet caft a glance upon this fubftance, and none of its cryftalline forms have hitherto been determined. Having had opportunities of examining fome of thefe forms, and at the fame time of comparing a number of fpecimens of this fubftance, procured from different countries, I felt myfelf induced to lay before the Public my obfervations upon this fubject, thinking they might contribute to throw fome light upon the nature of feveral ftones, with which we are not yet perfectly acquainted.

Its primitive form is a right prifm with rectangular bafes.

The primitive form of this fubftance, which has been defignated by the name of Anhydrous Sulphate of Lime by the Abbè Hauy, is a rectangular paralelopiped, as had been prefumed by this able mineralogift; but it does not appear that this paralelopiped can be the perfect cube, as is indicated by the habitual form of the cryftals, which when moft fimple, is always a right prifm with rectangular bafes, having two oppofite fides broader than the two others (Fig. 1, Plate XI.), and feems equally to be indicated by the circumftances which accompany this form.

Eafily divided.

The mechanical divifion is very eafy upon all the faces of this cryftal; but the longitudinal ftraight faces prefent, in this refpect, a little more refiftance than the others.

* And afterwards revifed by him.

The

The broad faces, as well as thofe which terminate this rectangular prifm, have generally a very brilliant luftre; the longitudinal narrow ones faces are duller, and very frequently ftriated in the direction of their length. The broad fides are befides characterifed by a pearly reflection, fimilar to that which is peculiar to the zeolite ftilbite, and this brilliancy remains even after thofe faces have been divided: they alfo habitually exhibit the interfection (*(entrecroiffement)* at right angles of the joints belonging to the other faces. Luftre.

This cryftal prefents, along its longitudinal edges, a decreafe which replaces each of thefe fides by a plane unequally inclined upon the adjacent faces: it forms with the broad fides of the prifm an angle of 130°, and of 140° with the narrow fides, (Fig. 2.) The prifm is therefore octahedral, and has four edges of 130°, and four others of 140°. Secondary forms. Octahedral prifm.

Thefe new planes frequently join each other, upon the narrow fides of the prifm, and they then convert the octahedral into a hexahedral form, having four edges of 130°, and the two others of 100° (Fig. 3.) The pearly fides frequently then continue to be the broadeft; in other inftances, the fix fides of the prifm are equal, or nearly equal with each other. I have not yet feen any fecondary faces placed upon the terminal edges. Hexahedral prifm.

The cryftals upon which this defcription is founded are of confiderable magnitude: fome are more than an inch in length. They are of a fine flefh colour, and formed part of one of the fpecimens brought from the Tyrol, in which the anhydrous fulphate of lime is mixed with muriate of foda or common falt; but as this falt is intirely foreign to it, and only interpofed between its parts, frequently even in a manner that is very perceptible to the eye, the form of thefe cryftals ought to be confidered as being really that of the pure anhydrous fulphate of lime. Befides, I have feen in Mr. Greville's cabinet at London, a fpecimen in which this fubftance prefents exactly the fame afpect, and the fame colour, and which at the fame time is totally deftitute of falt. In this, the anhydrous fulphate of lime is confufedly intermingled with actinote of a pale green colour, with fome parts of cupreous pyrites, and of the black and very magnetic oxide of iron. The locality of this interefting fpecimen has not been preferved; but there is every reafon to prefume that it came from Sweden. Foreign admixture of muriate of foda.

In

Groups of crystals; easily separated.

In the Tyrolese specimens, similar to that which afforded the crystals which I have just described, these are for the most part strongly engaged and applied against each other, crossing each other in different directions; some of them, however, are insulated. But as many of them are united by their broadish or pearly faces, they have no very strong adhesion, and it is always sufficiently easy to separate them with the edge of a knife. These specimens frequently exhibit in their mass more or less considerable portions of pure common salt; several of their crystals are considerably transparent.

Peculiarity. The presence of sulphur of antimony and quartz.

A piece of this substance which I procured for Mr. Chenevix, for the purpose of analysing it, exhibited a peculiarity which deserves to be remarked. On breaking some of its crystals, small needles of sulphuret of antimony were perceived within it, adhering for the most part to small groups of crystals of quartz. Not a trace of either of these two substances was found in the other parts that were subjected to analysis: the same was the case with the carbonate of lime, which Mr. Klaproth has indicated at $\frac{11}{100}$ of the analysis which he made of it, and which, undoubtedly, was likewise only an extraneous or interposed substance.

Quartz in another specimen.

In like manner, in the stone of Vulpino, observed and described very accurately and carefully by Messrs. Fleurieu and Bellevue, in the 2d Vol. of the Journal de Physique for 1798, the anhydrous sulphate of lime is mixed with interposed particles of quartz, which, according to the analysis made of it by Mr. Vauquelin, are in the proportion of $\frac{16}{100}$ to its whole mass. I am indebted to the friendship of Mr. Fleurieu for two specimens, which present two distinct varieties of this interesting stone.

One of them is of a good blue colour, is partly of a very fine sandy grain, and partly coarser and lamellated: it greatly resembles the carbonate of lime known by the name of saline marble. An immense number of small laminæ are observable in it, which cross each other in different directions, and are found by the magnifier to be perfectly rectangular.

The other is of a darker ash coloured grey; it is less pure than the former: some portions of a true gypseous earth are observed in it, containing a small quantity of argill and of carbonate of lime. Its substance is more compact than that of the

the preceding ſpecimen; its laminæ are larger, their rectangular form is more perceptible, and ſeveral of them have a pearly reflection.

Of two other pieces of the ſame ſubſtance, in the cabinet of Mr. Greville at London, which appear to me to belong to the anhydral ſulphate of lime of the ſalt-works in the canton of Bern, and both which are perfectly pure, the one is white with a little of a blueiſh caſt, the other has the ſame blueiſh tinge, but deeper, and greatly reſembles in colour the pale blue ſapphire, known by the name of water ſapphire. Both have a coarſe granulated texture, and are compoſed of a combination of rectangular laminæ, which croſs each other in different directions, as may eaſily be diſcerned with the naked eye. But the laminæ of that in which the blueiſh colour is the moſt intenſe, preſent beſides ſo lively a pearly reflection, that it might eaſily, at firſt ſight, be miſtaken for a maſs of zeolite ſtilbite. Rectangular laminæ, of ſome thickneſs, may be ſeparated from it, which, like the cryſtals before deſcribed, are eaſily divided in every direction. Other ſpecimens.

The ſpecific gravity of the anhydrous ſulphate of lime, mixed with common ſalt, proved to be 2940. That of the ſpecimens which I mentioned in Mr. Greville's cabinet, namely, that in which the laminæ had the ſtrong pearly reflection, was 2957, and the other 2929. Of the two of Vulpino, the moſt compact was 2951, and the other 2933. Specific gravity.

The hardneſs of this ſubſtance, in all the ſpecimens, is ſomewhat ſuperior to that of the carbonate of lime; in all of them alſo the anhydrous ſulphate of lime is fuſible by the blow-pipe without ebullition, and affords an opaque glaſs. Hardneſs.

There is a marked difference between them, with reſpect to the phoſphoreſcence, upon an heated ſhovel. The anhydrous ſulphate of lime of Vulpino, gives a pretty ſtrong orange-coloured light; that mixed with common ſalt, affords a very faint bluiſh light; that of the ſalt-works of Bern, none at all; and laſtly, that which I mentioned as containing actinote, with attractive oxide of iron, &c. gives a light ſomewhat more reddiſh than that of Vulpino. Phoſphoreſcence.

Gypſum deprived of water, or anhydrous ſulphate of lime, therefore, poſſeſſes characters altogether different from thoſe of the ſulphate of lime which contains that liquid; and as in theſe two ſtones the ſulphuric acid and the lime exiſt in the The abſence of water renders the properties altogether different.

ſame proportions, it cannot be doubted but that the preſence of the water totally changes the nature of the combination of acid and earth when it comes to be joined with it, which, it appears to me, can only take place ſo far as this water becomes itſelf an eſſential component part of the ſtone.

Remarks in proof that water is eſſential to the compoſition of many ſubſtances.

This is not the only ſubſtance which enables us to aſcertain that the water, which formerly was conſidered as a part foreign to the ſtones which contained it, becomes in reality, in ſeveral of them, an ingredient eſſential to their nature. The analyſis which Mr. Chenevix has made of the different ſpecies of arſeniates of copper, have preſented us a ſtriking example of this, eſpecially in that variety of the third ſpecies of my deſcription (ſee Philoſophical Tranſactions, 1801) to which I have given the name of Hematiform. A movement of decompoſition, which is confined to the gradual loſs of their conſtituent water, totally changes the colour of theſe arſeniates, and at laſt completely diſcolours them, at the ſame time that it renders thoſe that before had ſome feeble tranſparency, perfectly opaque. This loſs of the water always commences at the exterior, and in this caſe the interior part preſerves all its tranſparency as well as its colour, whilſt the exterior part is diſcoloured, and exhibits by the ſhrinking of its ſurface, ſometimes to a very conſiderable degree, ſenſible marks of the loſs which it has ſuſtained; the water amounting to about one-fifth of its maſs *.

Arſeniates of copper.

* In my deſcription of the arſeniates of copper quoted above, I have conſidered this hematiform arſeniate, as well as thoſe which I have deſignated by the terms of indeterminate, capillary, and amianthiform, only as being varieties of the ſpecies in the acute octahedral form: the copper and the arſenical acid are in fact contained in them in the ſame proportions; but the water, which adds a new conſtituent part to them, and which did not exiſt in thoſe varieties of this third ſpecies, which are in perfectly determined cryſtals, forms with them a real hydrate of this third ſpecies. I therefore think that it would be proper to ſeparate theſe varieties, in order to form with them a fifth ſpecies perfectly diſtinct from the third. The arſeniate of copper is one of the moſt aſtoniſhing productions of the mineral kingdom, by the immenſity of the aſpects under which it preſents itſelf, all which, nevertheleſs, have certainly a particular cauſe, which I am very far from pretending to have aſcertained.

In

In the ſame manner it is, that, in the blue carbonate of copper, which, according to the inveſtigations of Mr. Prouſt, appears to contain a conſiderable quantity of water, far ſuperior to that which exiſts in the green carbonate, the cryſtals ſeem to paſs, at their ſurface, into the ſtate of green carbonate, by the mere loſs of a certain portion of their conſtituent water. This ſpecies of decompoſition is ſometimes even ſo conſiderable in them, that it exiſts, for example, amongſt the ſpecimens in large and ſuperb cryſtals which come from Siberia, cryſtals, of which the form having undergone no change, belongs to the blue carbonate, but which have entirely paſſed, throughout their whole ſubſtance, into the ſtate of green carbonate.

Blue carbonate of copper.

I am fully perſuaded, that when water is once conſidered and admittted as a conſtituent part in the compoſition of numerous mineral ſubſtances, it will ſoon be acknowledged to contribute greatly, by its preſence or its abſence, to the difference which ſubſiſts between ſeveral ſtones: thus I am ſtrongly inclined to believe that the carbonate of lime of ſlow ſolution, the hardneſs and ſpecific gravity of which are ſo much ſuperior to the ſame characters in the ordinary carbonate of lime, differs from this latter perhaps only by a difference in the water of compoſition. Moſt certainly this difference cannot proceed either from the preſence of argil, or from that of magneſia. I know ſome dolomies which certainly do not contain any trace of argil, whilſt, at the ſame time, I know carbonates of lime, which are highly charged with magneſia, and which, nevertheleſs, are very ſpeedily diſſolved in the acids: Of the number of theſe latter are, for example, moſt of the calcareous ſpars with a pearly reflection and greaſy aſpect.

Water is of great importance as an ingredient which modifies the properties of bodies.

I am much inclined alſo to think, that the ſpecies of Chalcedony, named cacholong, may owe its difference from the common chalcedony only to the circumſtance, that the latter contains water of compoſition of which the chacolong is deſtitute; and that the tranſition of the chalcedony into this ſtate, and ſubſequently into the hydrophanes, depends, in a great meaſure, upon the loſs of this water. The probability of this opinion remains to be ſettled by future obſervations.

Cacholong probably owes its peculiar properties to the abſence of water.

IX.

Analysis of Natural and Artificial Anhydrous Sulphate of Lime, by RICH. CHENEVIX, *Esq. F. R. S. M. R. I. A. Communicated by the Author.*

Component parts of common sulphate of lime.

THE proportion of the elements in common sulphate of lime, such as I have stated them in the Transactions of the Royal Irish Academy, are somewhat different from those given by former chemists. Mons. Fourcroy, in his "*Système* "*des Connoissances Chimiques*," and in his "*Synoptic Tables*," has determined them in the following order: sulphuric acid 45, lime 32, water 22. If we deduct the quantity of water, and reduce the remainder to the quintal, we shall have the following proportions: sulphuric acid 58,5, lime 41,5. As the object of the present Paper is merely the Anhydrous sulphate, I shall not take further notice of the quantity of water that may be contained in common sulphate of lime, whether natural or artificial.

Lime dissolved in muriatic acid and sulphuric acid added, and the whole exposed to violent ignition, left dry sulphate of lime containing 56,3 lime, and 43,6 acid.

I took one hundred parts of lime, prepared with the greatest care, and dissolved them in muriatic acid. I then poured sulphuric acid into the solution, and heated the whole in a platina crucible, at first gently, but afterwards to violent ignition. The augmentation of weight in the lime and the crucible, (which had been weighed before the operation) amounted to 78,5, and was combined sulphuric acid. This experiment therefore indicates, that strongly calcined sulphate of lime is composed of lime 56,3, and 43,6 of sulphuric acid.

Sulphate of barytes contains 24 acid in the 100 parts.

I then took one hundred parts of calcined sulphate of lime, and decomposed them by oxalic acid, to render them more soluble, and then dissolved them in muriatic acid.

I precipitated the solution by muriate of barytes, and obtained 182 of sulphate of barytes. Hence it is evident that 182 of sulphate of barytes, and 100 of calcined sulphate of lime contain the same quantity of sulphuric acid $= 43,6$; which proportion gives 24 as the quantity of sulphuric acid, in 100 of sulphate of barytes. This quantity is nearly intermediate between that given by Thenard, and that which I had already stated. By this experiment I established a standard, to which I might refer every kind of sulphate of lime.

 It

It is evident that calcined fulphate of lime, or artificial anhydrous fulphate of lime, contains 43,6 of fulphuric acid. It remains to prove its chemical identity, with the natural anhydrous fulphate.

I took one hundred parts of this fubftance, in as pure a ftate as the Count de Bournon could procure it, and fubmitted it to the fame experiments. I obtained from this experiment 187 of fulphate of barytes, which announces 44,88. I fhall never expect more uniform refults in two analyfes, even of the fame fubftance, and do not hefitate to pronounce, that the two fulphates are, chemically fpeaking, one and the fame thing.

The natural anhydrous fulphate of lime was treated like the artificial, and gave 44,88 acid.

The Abbé Hauy in his late work has given the proportions in anhydrous fulphate of lime, according to an analyfis of Monf. Vauquelin. This excellent chemift found its elements to be nearly in the inverfe order of my ftatement, and fuch as Fourcroy has eftablifhed them for the artificial fulphate, deducting the water of cryftallization. I fhould ftill have doubted the accuracy of my experiments, if I had not difcovered a caufe that may explain the difference which exifts between his refults and mine.

Vauquelin's experiments gave much more acid;

The French chemifts have mentioned two varieties of fulphate of barytes, one of which contains 13 per cent. of fulphuric acid, and the other 33. If therefore we eftimate the quantity of acid contained in fulphate of lime, by the fulphate of barytes containing 33, and not by that containing 24 per cent. we fhall have a much greater quantity of fulphuric acid, as a conftituent part of fulphate of lime.

moft probably from his affuming a higher proportion of acid in the fulphate of barytes.

Among the fpecimens which the Count de Bournon gave me for trial, there were fome which contained muriate of foda. This falt was eafily extracted by water alone; and the proportion of it differed in different fpecimens. Klaproth had fome fpecimens in which he found carbonate of lime, and even filica. But as I have examined fome in which I can pofitively affert that there was neither the one nor the other, thofe fubftances may be looked upon as merely accidental.

Some fpecimens of the anhydrous fulphate contained muriate of foda;

but it was accidental.

X. *Abridgment*

X.

Abridgment of a Memoir of Mr. PROUST *on Tanin and its Species* *.

Uncertainty of the proceſs of ſeparating tanin by muriate of tin.

THE proceſs which Mr. Prouſt has given for ſeparating Tanin by muriate of tin is ſubject to conſiderable uncertainty when it is uſed, as an Engliſh Chemiſt has lately done to fix the proportion of this principle in vegetable juices. He thinks, therefore, that it may be of utility to announce theſe cauſes of error, in order that philoſophers, who are engaged in this department of reſearch, may place leſs confidence in it, and turn their thoughts to ſome more perfect method. To theſe remarks he adds an account of certain varieties which he thinks he has perceived in the genus of Tanin.

When the oxide of tin is ſeized by the tanin, the muriatic acid inſtead of being ſet at liberty diſſolves part of the tannate.

1. When this muriate is ſaturated with an aſtringent juice, it happens that the muriatic acid takes up in ſolution a portion of the tannate of tin, ſo that what is collected on the filter repreſents, in truth, only a part of the tanin principle contained in the plant. This effect is ſimilar to what happens in the preparation of ink, the black dye, and in every caſe where a dyeing principle can deprive an acid of the oxide which it held in ſolution. The affinity oft his acid not being capable of remaining inactive exerts itſelf upon the coloured oxide.

This tannate may be thrown down by careful additionof alkali.

When ſmall doſes of alkali are added to the liquor the reſidue of the tannate may be made to precipitate without even touching the gallic acid if it be preſent; and if this point be exceeded it may ſoon be perceived by the green colour which the fluid receives from contact of the air. In that caſe a few drops of acid will be ſufficient to diſpel the cloud by ſeizing the exceſs of alkali and ſetting the gallic acid at liberty. But as, on the other part, it is neceſſary at firſt to employ an exceſs of muriate to aſſure the precipitation of the whole of the tanin, there is danger of loading the tannate with a certain exceſs of oxide of tin.

But the tannate will be overcharged with oxide if too much of the muriate has been uſed.

The muriate precipitates extractive matter and perhaps other principles along with the tanin.

2. The preſence of tanin does not exclude the extractive principle in the juice of a plant, and as the muriate precipitates this laſt as well as the former, the extractive principle will therefore become a new ſource of miſtake in the eſtimate of the

* From the Annales de Chemie XLII. 89.

the quantity of tanin. It may alſo happen that the vegetable juices may contain many other ſubſtances capable of decompoſing the muriate either directly or indirectly, ſo that this re-agent can never be depended upon with confidence.

There are beſides earthy ſalts in theſe juices as Vauquelin has ſhewn. Some, as for example Sumac contain ſulphate of lime in abundance. If therefore we uſe the alkali to complete the ſeparation of the tannate, the precipitate will alſo be charged with an earthy depoſition.

When earthy ſalts exiſt in the vegetable, theſe will let fall their baſes when the alkali is added.

3. On reflecting upon the means of avoiding the errors cauſed by the muriatic acid, Mr. Prouſt tried a proceſs which he had ſucceſsfully uſed to ſeparate the colouring principle from the gelatinous mucilage which accompanies it in cochineal. It conſiſts in heating, or even ſimply agitating, the aſtringent juice with the oxide of tin prepared by nitric acid, and kept under water. The oxide in fact, becomes loaded with tanin in a few days. But if the juice of a plant which is not aſtringent, or a diluted extract, be treated in the ſame manner, it will alſo become deprived of the whole of its extractive matter: and the gum and the ſalt will remain alone in the fluid. This method cannot therefore lead us to our object.

If the pure maximum oxide of tin be heated or agitated with tanin a combination takes place and, mucilage, if preſent, is left:

but this proceſs will not ſeparate the extractive matter.

4. What appears moſt ſurpriſing to this chemiſt in the preſent experiment is the deſtruction of the gallic acid, or probably its tranſition into a ſtate in which it cannot perform the functions of that acid. The fluid in fact when cleared of the tannated oxide by the filter has no longer either colour or taſte, and makes not the ſlighteſt impreſſion on the ſolutions of iron, nor even upon turnſol. When examined by every trial the fluid is found to be mere water.

Singular event in the laſt experiment. The gallic acid is deſtroyed.

5. Suſpecting, nevertheleſs, that this acid might be combined with the oxide of tin, he paſſed the tannate of the laſt deſcribed proceſs into potaſh. The product was a coloured fluid, in which he found no ſign of the gallic acid: for it did not aſſume by expoſure to the air that green ſhade which it always indicates when ſaturated with an alkali. He precipitated the tanin by a diluted acid, a portion remaining in ſolution, as happens in this caſe and he proceeded to examine that which remained upon the filter; but he ſoon perceived that it had alſo advanced towards a ſtate in which, as we ſhall ſhew, he found infinitely leſs tanin than before.

When the tanate &c. was decompoſed by potaſh no gallic acid was found and much leſs of tanin.

6 Boiling

The tanin that is ſeparated is not totally ſoluble in boiling water; neither does it precipitate glue, nor has it the taſte or ſmell of tanin &c. It reſembles extracts.

6. Boiling water cannot totally diſſolve it. Its ſolution no longer precipitates glue; it has neither the harſh taſte nor the odour of tanin. With the red ſulphate of iron it affords only a whitiſh grey precipitate, and laſtly it does not afford a magma with the muriate of tin; it is precipitated merely in the manner of extracts, to the taſte of which it in ſome meaſure approaches though it does not poſſeſs their bitterneſs. Theſe are the alterations to which the taning principle is ſubject when combined as before mentioned with tin oxided to the maximum.

It was ſuſpected that the tanin had taken oxigen from the oxide; but experiments on the latter did not ſeem to indicate this loſs.

7. From theſe changes he ſuſpected that the tin might have yielded to it that portion of oxigen which conſtitutes the difference between the oxide at the maximum and the oxide at the minimum, as happens with the oxide of iron in ink, hermetically cloſed. In order to aſcertain this he diſſolved in muriatic acid the oxide what had been deprived of tanin by potaſh; but he diſcovered no indication of that kind. The ſolution produced no change in that of gold, nor in corroſive ſublimate. It was at the maximum. It is true that with reſpect to the oxide of tin at the minimum, as well as that of iron, waſhing and expoſing to the air ſpeedily bring it to the maximum.

Theory of this converſion yet unknown.

Whether by oxidation or by whatever other proceſs, the tanin principle did at length paſs to the ſtate of ordinary extract. Tanin precipitates glue, but extract does not; this is the difference between them, The influence of ſome affinity, which the author has not ſufficiently developed, muſt have changed its radicals in their primordial arrangements or in their proportions; and it may be ſuppoſed that the gallic acid likewiſe after having been ſubjected to theſe changes became aſſimilated in the ſame ſtate and by the ſame cauſes.

Concerning the Varieties of Tanin.

The genus tanin compoſed of ſpecies.

If in the ſeries of immediate principles which compoſe the whole of vegetable matter we conſider the tanin of galls as a genus, becauſe this in fact poſſeſſes the qualities in the higheſt degree, it is eaſy to form a notion that this genus may have its ſpecies, and may, as well as ſugar, gum, ſtarch, &c. affect different modifications. There are various ſpecies of ſugar, reſin, gum &c, and there may be alſo various ſpecies of tanin. Mr. Prouſt thinks that he has found this to be the caſe.

Cachou

Cachou, or Terra japonica.

Qualities of the various species of tanin.

Cachou or Terra japonica.

Cachou is an aftringent: it is foluble in alcohol and in water. It precipitates glue abundantly and forms with it a vinous (q.) magma which has neither the confiftence nor the infolubility of the tannate of galls.

It reduces muriate of gold, it precipitates the muriate of tin and affords a violet coloured ink with red fulphate of iron. It is a tanin *fui generis*. It dyes filk.

Dragons Blood.

Dragon's Blood.

That which is pure and comes to us in calebafhes is foluble both in water and in alcohol; its tafte is harfh; it dyes filk of a bad wine colour. It abundantly precipitates glue, the muriate of tin, the red fulphate, and difoxides gold; it is alfo a fpecies of tanin.

Sumach.

Sumach.

The tanin which this fubftance contains, abundantly precipitates glue, and affords a white magma without confiftence. Like tanin it is feparated from the decoction of fumach by the carbonate of potafh; its curdled depofition is again foluble in hot water with the exception of a fmall quantity of chalk.

Barites and the oxalic acid demonftrate the exiftence of abundance of lime and fulphuric acid; but it will be neceffary to afcertain by experiments on the green fumach whether fulphate of lime be one of its principles, or an adulteration. The author is not furprifed at finding it, fince he obtained this falt in confiderable quantity from the juice of cabbage, and of folanum lycoperficon, which is cultivated in the gardens under the name of lomates. This juice alfo contains gallic acid; it becomes green in the air when faturated with potafh. It reduces gold, decompofes muriate of tin and the red fulphate, with which it affords a thick ink.

Yellow Wood, or Fuftic.

Yellow wood or fuftic.

It contains a fpecies of tanin. Like tanin it precipitates the folution of glue; a folution of falt is fufficient to precipitate it. It is foluble in water and in alcohol. It reduces gold, decompofes muriate of tin and the red fulphate, by means of which it dyes filk of a greyifh yellow.

Fuftet

Fustet.

Fustet.

It is a pure dyeing extract soluble in water as well as in alcohol. It contains a small quantity of gallic acid, but does not change the solution of glue. It reduces gold, precipitates the metallic salts, and has no gummy portion.

Grains of Avignon, or French Berries.

Gr. d'Avignon, or French berries.

They afford a dyeing extract of the same nature, without gum or tanin. It reduces gold, &c.

Brazil Wood.

Brazil wood.

Affords also a dyeing extract soluble in alcohol, without tanin, or gum, reducing gold and precipitating the metallic salts.

Reduction of gold is no exclusive character of tanin.

Mr. Proust has remarked, that the reduction of gold has ceased to be a characteristic quality, since he observed the muriatic solution abandon this metal to all the tinging substances, such as anise, cochineal, gum guttæ, gallic acid, verjuice, wine, vinegar, the juice of all fruits, manna, gum, and sugar, though somewhat slowly.

Concluding observations.

The author concludes by observing, that tanin has its varieties like the other immediate products; that the property of precipitating glue is the generic indication, by which they are distinguished from extracts, which do not alter that substance; and lastly, that the different species of tanin, particularly those which have been discovered in the barks of trees, cannot be compared together as to their force and their useful qualities, but by observations upon skins which have been submitted to their action.

Taning matters *require to be tried by actual preparation of leather.*

Sulphate of lime very common in vegetables.

Note. Plaster must be infinitely common in vegetables. Mr. Proust has found it in verjuice, grapes, apples, gooseberries, &c.

B. L.

XI. *Description*

XI.

Description of an Apparatus for heating Water by waste Steam. Invented by Mr. ARTHUR WOOLF.

THE following apparatus was erected at the extensive brewery of Messrs. Meux and Co. in August, 1800, and has been in use ever since. I saw it work a few weeks ago, and observed with great pleasure the facility and precision with which it operates, and I have great satisfaction in presenting it to the reader as a very judicious and useful combination. **Engine for heating water by steam.**

Plate IX. A represents a steam pipe from the brewing copper. **Particular description.**

B a valve with its weight.

C the vessel in which the steam is condensed.

D a pipe that conveys the cold water from a reservoir.

E a conical valve through which the water is injected. It is connected with the lever F.

G is a bended pipe to prevent any of the steam from escaping with the hot water.

H a small receiver from which the hot water may be conveyed to different situations by means of pipes and cocks.

I a pipe open to the receiver to prevent a vacuum in case the water should be made to descend in any of the pipes.

K a small pipe to convey the steam into the regulator.

L the regulator which is composed of three cylinders, the outside and inside being closed together at bottom, leaving a cavity between, which is filled with water; the middle or moving cylinder is inverted and close at top. It serves for a piston, and is connected to the lever M, on which is a sliding weight N, by which the quantity and heat of the water may be varied at pleasure.

O is a valve through which the steam is let out when not used for heating water.

The effect of this engine may be easily understood. The weight of the inverted hollow piston L presses down the valve E by means of the levers, and this pressure may be regulated by fixing the weight N nearer or farther from the centre of the upper lever. When the steam through A has acquired a certain degree of strength in the vessel C, it raises the piston by its action through K, and consequently opens the valve E. A sheet of water immediately dashes through, as represented **Explanation of the manner in which it acts.**

 in

in the figure, and by condenſing the ſteam, ſuffers L again to deſcend; and, after a vibration or two, the effect of the ſteam to raiſe the piſton and of the injection to depreſs it, balance each other, ſo that the levers remain nearly motionleſs. It is evident that the injection will be leſs, the ſteam ſtronger, and the water which paſſes off through G hotter the nearer the weight M is to the centre of motion. And in this reſpect the apparatus is ſo effectual that the water may be heated to 210 degrees, and the quantity that paſſes off is from 100 to 160 barrels per hour, according to the temperature, as governed by the poſition of the weight M.

XII.

Deſcription of an improved Drawback Lock for Houſe Doors. By Mr. WM. BULLOCK. *From the Tranſactions of the Society of Arts, who adjudged a Reward of Fifteen Guineas to the Inventor.*

Copy of a Letter to the Secretary of the Society Mr. CHARLES TAYLOR.

SIR,

Inconveniences and danger of the common drawback lock.

I HAVE herewith ſent, for the inſpection of the Society, an improved Drawback Lock for Houſe Doors, &c. which improvement is in latching the door; for it is well known, particularly in damp weather, that the air drawing through it, ruſts the head or bevel of the bolt, by which means it requires great force to ſhut the door, and occaſions a diſagreeable noiſe, beſides ſhaking the building.

It has frequently happened that the houſe has been expoſed to robbery from the door being left unlatched, when ſuppoſed to be faſt. This improvement removes all thoſe inconveniences, as it lets the bolt ſhoot into the ſtaple immediately when the door cloſes, but not before; and the reliever works ſo very eaſy, that the door is made faſt with one twenty-fourth part of the force required with locks upon the common conſtruction.

By

By an experiment with the lock fent herewith, it will be proved that two ounces added to the reliever, will fhoot the lock with more eafe than three pounds will do, applied to the bevel bolt; and if the lock is rufty, the advantage will be much more in favour of the new method. I flatter myfelf it will be of great utility to the public, as its conftruction is fimple and cheap. It may be added to any old lock, as may be feen from that now fent. It may be advantageoufly applied to French windows and glafs doors, as it prevents the door from being ftrained, or the glafs broke, by the force applied to fhut them. I have fixed feveral locks, upon this new principle, which anfwer well; and if the invention meets with the approbation of the Society, I hope to be rewarded according to its merit. Advantages of the improved lock.

I remain, with refpect,

SIR,

Your moft obedient Servant,

WILLIAM BULLOCK.

No. 6, Portland Street, Soho, May 5, 1801.

Plate X. Fig. 2. A. Is the new iron latch here affixed to an old common drawback houfe lock. Defcription

B. An iron pin at one end of the latch, on which pin it is moveable.

C. A projecting part of the latch, which, when the common fpring bolt D of the lock is drawn back, in the ufual manner, is forced into the nick on its higher part at E, by the fpring F, underneath the latch.

The bolt D then remains within the lock, until, on clofing the door, the reliever G gently preffes on the lock box, fixed in the common way on the door cheek; which preffure draws the projecting part C out of the nick E, and permits the end of the bolt D, by the force of the fpring G, to flide into the lock box, and faften the door.

XIII. *Defcription*

XIII.

Description of an improved Mill for grinding hard Substances. By Mr. GARNETT TERRY. *From the Transactions of the Society of Arts, who adjudged the Silver Medal to the Inventor.*

Description of an improved mill.

MR. Terry, whose residence is No. 20, City Road, Finsbury Square, has constructed this mill on a large scale, and there is also a model deposited in the Society's collection.

Plate X. Fig. 1. A. The hopper, or receptacle of the articles which are intended to be ground.

B. A spiral wire, in the form of a reversed cone, to regulate the delivery of them.

C. An inclined iron plate, hung upon a pin on its higher end: the lower end rests on the grooved axis D, and agitates the wire B.

D. The grooved axis, or grinding cylinder, which acts against the channelled iron plate E.

F. A screw on the side of the mill, by means of which the iron plate E is brought nearer to or removed further from the axis D, according as the article is wanted finer or coarser.

G. The handle, by which motion is given to the axis.

H. The tube from whence the articles, when ground, are received.

*** The front of the mill is taken off, in order to show its interior construction.

XIV.

Remarks on Dr. Thomson's Theory of Combustion. By C. R.

(Received June 15, 1802.)

General remarks on Dr. Thomson's theory, and the distinction between combustion and oxigenation.

THE scientific world are so highly indebted to Dr. Thomson for many original communications, and for the very perspicuous manner in which he has explained many of the phenomena of chemistry, that every thing that is presented to the world under the sanction of his name, is intitled to much consideration: if in some instances we are induced to hesitate in the yielding of our assent, we cannot but do justice to the ingenuity of his reasonings, and at the same time acknowledge the

the very luminous manner in which he conveys information, on every ſubject that he treats. The Paper under conſideration is particularly an inſtance in point, and if we cannot go the full length with the author, we muſt at leaſt acknowledge, that in the chief, his diſtinctions are accurate, and his reaſoning juſt. Nothing can be more evident than the difference which in numberleſs inſtances prevails, between the act of oxigenation in bodies, and that of combuſtion, inaſmuch as neither the phænomena attending them, nor the reſults ariſing therefrom, are the ſame. The French chemiſts, however, ſeem to have been miſled, in their confining the term combuſtion to the act of oxigenation, by conſidering, that all bodies during their combuſtion combine with oxigen, without at the ſame time recollecting, that this latter effect may be produced without any of the phænomena uſually attendant on combuſtion, and that though certainly all combuſtion preſuppoſes the combination of oxigen with a baſe, yet this combination may, and repeatedly is effected where no combuſtion can poſſibly take place.

That a diſtinction therefore prevails between the two is obvious, and the Doctor offers us a theory, which he conſiders as ſufficient to explain the different phænomena produced.— This theory it is the purpoſe of the following lines to ſhew however ingenious, aud apparently ſatisfactory it may appear to be, is not wholly adequate to the taſk that is aſſigned to it.

Outline of the theory. That the light of combuſtion is furniſhed by the combuſtible body and the heat by the oxigen of ſupporters;

It will be neceſſary very ſhortly to ſtate here the outline of the theory under conſideration. In all caſes ſays Dr. Thomſon *, when heat and light are extricated during combuſtion, it will be found, that the light is furniſhed by the combuſtible or burning body, and the heat by the decompoſition of the oxigen, which forms a component part of the ſupporter, and which is eſſential to the combuſtion, and that the diſtinction that prevails between the two proceſſes of combuſtion, and of oxigenation, ariſes from the difference of the phænomena, which accompany the action of ſupporters and products upon other combuſtibles. "The ſupporters convert theſe bodies into products, and combuſtion, or the emiſſion of heat and light at the ſame time take place; whereas, the products convert combuſtibles into products, without any ſuch emiſſion.

but that products convert combuſtibles into products *by mere oxigenation without combuſtion.*

* Philoſ. Journal, New Series, II. 10 and 92.

Now

Now as the ultimate change produced on combuſtibles by both theſe ſets of bodies is the ſame, and as the ſubſtance which combines with the combuſtible is the ſame, namely oxigen, it is evident, that the oxigen of the ſupporters contains ſomething, which the oxigen of the products wants," and this ſomething the Doctor ſuppoſes to be caloric. "In the ſame manner the combuſtibles and products reſemble each other, the chief difference between them conſiſting, in the phænomena which accompany their combination with oxigen, in the one caſe fire is emitted, and in the other not." Now ſays the Doctor, "if we recollect, that no ſubſtance but a combuſtible is capable of reſtoring combuſtion to the baſe of a product, and that at the time of its doing ſo, it always loſes its own combuſtibility, and further, that the baſe of a product does not exhibit the phænomena of combuſtion even when it combines with oxigen, we cannot avoid concluding that all combuſtibles contain an ingredient, which they loſe when converted into products, and that this loſs contributes to the fire, which makes its appearance during the converſion." This ingredient the Doctor ſuppoſes to be light.

Leading poſitions of the theory, 1. that light is originally an ingredient of combuſtibles, 2. and heat of oxigen.

It is evident, that the two leading poſitions of this theory are, 1. That during combuſtion, all combuſtibles emit light, which previouſly formed a neceſſary ingredient to their own compoſition; and ſecondly, That the heat which is evolved during the proceſs of combuſtion, proceeds from the oxigen of the ſupporter, of which it likewiſe originally formed an eſſential ingredient.

Many reaſons why the heat muſt come from the oxigen of the ſupporter.

—That the heat given out during combuſtion comes from the decompoſition of the oxigen of the ſupporter, there are many reaſons for concluding. We know very well that no combuſtion will take place without the preſence of oxigen, and that the greater the quantity of oxigen abſorbed in a given time, the greater is always the heat that is evolved. Now if the heat be not ſuppoſed to come from the oxigen, why ſhould the degree of heat given out, be always proportional to the quantity of oxigen that is abſorbed, and upon what other principle can we ſo ſatisfactorily explain the effects that are produced by the Argand lamp. Theſe conſiderations, combined with the argument drawn from the maintenance of the temperature of hot blooded animals by the decompoſition of air, ſeem ſufficiently to eſtabliſh the truth of the foregoing poſition.

Difficulties as to the other poſition, that the light invariably

There are, however, many difficulties that preſs againſt our implicit adoption of the other

other part of this theory, *viz.* that the light emitted during combuftion invariably proceeds from the burning body, and that confequently it forms no part of the fupporter. It of courfe then follows from this theory, that light is no effential part in the compofition of oxigen gas. Let us however fee whether this be the cafe. Many facts it will be found concur to prove that the contrary is the truth. If nitric acid be expofed to the light, after fome time we find that it changes colour, it becomes yellow, green, and then red, and oxigen gas is difengaged, the nitric at the fame time being converted into nitrous acid. Now it is evident, that as this decompofition is of a chemical nature, the light that occafions it, either combines with the oxigen to form oxigen gas, or with the acid to form nitrous acid: as we find no diffimilarity between the nitrous acid procured by this means, or that by any other, we are neceffitated to conclude that the light has combined with the oxigen, and that the latter by the fame means is converted into oxigen gas. Again, it is well known, if oxigenated muriatic acid be expofed to the rays of the fun in a tranfparent bottle, there is difengaged from it oxigen gas; in proportion as the gas is feparated the acid lofes its colour and odour, and returns to the ftate of fimple muriatic acid.—Here it is evident, that the oxigen has paffed from a concrete into a gafeous ftate from the combination of light, and we muft therefore conclude that light is a component part of oxigen gas.

proceeds from the burning body.

For the light which decompofes nitric or ox. mur. acid is concluded to have combined with the difengaged oxigen.

If phofphorus be inferted in nitric acid, the latter is decompofed, and a product of combuftion, namely phofphoric acid is formed, during which procefs neither heat nor light are given out. This procefs Dr. Thomfon confiders as an act of oxigenation, and not of combuftion, becaufe, fays he, though a product of combuftion is formed, a new fupporter, namely nitrous gas is evolved, and the formation of a combuftible, or new fupporter, conftitutes one of the characteriftic differences between the two proceffes of combuftion and oxigenation. Now it is faid, that in all cafes of oxigenation a double decompofition takes place, the oxigen of the product combines with the bafe of the combuftible, while the light of the combuftible combines with the bafe of the product. The queftion then naturally prefents itfelf,—what during this procefs becomes of the light which made a component part of the phofphorus

When phofphorus is acidified in nitrous acid, what becomes of the light of the combuftible?

phoſphorus previous to its converſion into a product? It cannot combine with the new ſupporter that is evolved, becauſe it is a part of this theory, that light is no conſtituent part of ſupporters, but only of combuſtibles; it ſhould therefore have been made evident to the ſenſes, which we do not ever find to be the caſe, nor is any heat evolved; this latter effect is no doubt very eaſily explained, but what becomes of the light yet remains to be ſhewn.

Sulphurous and ſulphuric acids.

Sulphuric acid, ſays Dr. Thomſon, is a ſubſtance which from many of its properties I conclude to be a combuſtible, and not a product. This concluſion, however, does not appear to be perfectly conſiſtent with the definition the Doctor has given of combuſtion, for when ſulphur is heated in the air to the temperature of 302 degrees, it gives out light and heat, and is converted into an acid, *viz.* ſulphurous acid: this according to the theory under conſideration is a complete act of combuſtion, and therefore only a product of combuſtion, and not a combuſtible body ought to be formed. Sulphurous acid, according to La Grange, combines ſlowly with oxigen, and is converted into ſulphuric acid, but as no light and heat are rendered viſible, ought it not in this caſe rather to be conſidered as an act of oxigenation; for if light and heat were evolved in this proceſs, it ſhould appear that combuſtibles are capable of giving out a part only of their light in ſome caſes, and the whole in others, which does not appear very probable, for it cannot be doubted but that in ſulphurous acid, the oxigen and the ſulphur mutually ſaturate each other, and that ſulphuric acid is only ſulphurous acid combined with an additional doſe of oxigen.

Decompoſition of water by iron or zinc.

Though the Doctor apparently reconciles the decompoſition of water by iron or zinc with his theory, it yet appears to be attended with ſome difficulties which are not eaſily explained.—"Whenever, ſays he, the whole of the oxigen is abſtracted from products, the combuſtibility of their baſe is reſtored as completely as before combuſtion, but no ſubſtance is capable of abſtracting the whole of the oxigen from ſuch products, except a combuſtible, or partial combuſtible. Water, for inſtance, is a product of combuſtion whoſe baſe is hidrogen; to reſtore the combuſtibility of the hidrogen, we have only to mix water with iron or zinc filings, when the metal is oxidated, and the hidrogen gas is evolved as combuſtible as ever." Let us here attend to the phænomena which

which ſhould take place according to the Doctor's theory; water, which is a product of combuſtion, is hidrogen without its light, in union with oxigen, without its heat; by adding iron (a combuſtible containing light) we decompoſe the water, that is to ſay, 73 parts of iron unite with 27 of oxigen. Now as to every 27 parts of oxigen in water there are about four of hidrogen, of courſe theſe four parts of hidrogen are liberated; but as it does not appear probable that combuſtibles ſhould be capable of combining with light in all proportions, it may be aſked, if the 73 parts of iron which are oxidated contain juſt light enough, and no more, to reſtore the combuſtibility of the four parts of hidrogen; for if there be too much for that purpoſe, the ſuperabundant quantity ought to become viſible, and if too little, a part only of the hidrogen ſhould recover its combuſtibility, and be converted into gas. And the ſame reaſoning may of courſe be urged with regard to the decompoſition of water by zinc: for it cannot but be acknowledged, that the fact is ſomewhat ſingular, that the product of combuſtion ſhould always contain and give out the preciſe quantity of light which is ſufficient to reſtore combuſtibility to the baſe of the product, and in no caſe either more or leſs. Thus it appears that there are many difficulties that attend our implicit aſſent to the preſent theory, and many of the phænomena of combuſtion that do not apparently admit from it of an eaſy interpretation. Whether Dr Thomſon can reconcile theſe apparent anomalies to it, remains to be ſeen; but if it is found equal to their ſolution, there could then it ſhould ſeem be little objection to its adoption. At all events, no one will be inclined to diſpute, that the Doctor has thrown much light on a ſubject, which before its inveſtigation by him, was conſiderably more obſcure: and that he has placed it in a new point of view, which bids fair to enable us to approximate much nearer to a true theory with regard to the phænomena of combuſtion, than any other that has hitherto prevailed.

Numerically conſidered as to the tranſition of preciſely adequate portions of light from combuſtible to combuſtible, &c.

Concluſion.

Dr. T. has thrown great light on this obſcure ſubject.

C. P.

XV.

On certain Points of Nomenclature. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

On the uſe of *y* in Engliſh for the Greek vowel *υ*.

APPREHENDING that your mode of writing certain newly impoſed names of ſubſtances in chemiſtry, ariſes from inattention, and being miſled by the French, I take the liberty of a friend to remind you, that, in Engliſh, it is not uſual to write the vowel *i* for the *υ* of the Greeks, but *y*; hence, in our language, we do not write ox*i*gen, h*i*drogen, ox*i*geniſed, &c. but oxygen, hydrogen, oxygeniſed, &c.: you write, however, properly, ox*i*de inſtead of ox*y*d, as ſome perſons ſpell the word; becauſe oxide ſhews the etymology in ὀξυς and εἶδος better than oxide.

Your's ever faithfully,

A. B. C.

ANSWER.

I DO not profeſs to have directed much attention to the ſubject of nomenclature; though I am well aware of its importance to the acquiſition, as well as the communication of knowledge. It is, therefore, with conſiderable diffidence that I ſtate my apprehenſion, that neither uſage in a language, nor the motive of preciſely indicating the derivation of a term, are very cogent arguments for adopting any particular mode of ſtructure, if other motives preſent themſelves. To me it ſeemed at leaſt as forcible a reaſon for the uſe of *i* inſtead of *y*, in the words alluded to, that, together with their derivatives, they are ſo very numerous, as to make it deſirable to accommodate them to the general uſage of the modern languages; and this appeared to be promoted by following the change propoſed by the framers of the chemical nomenclature.

XVI. *Duplicate*

XVI.

Duplicate Copy of a Letter from BARON DE ZACH *to the Right Honourable Sir* JOSEPH BANKS, *Bart. P. R. S. &c. tranſmitted to Mr.* EDWARD TROUGHTON, *and communicated by the Rev.* J. PEARSON; *on the new Planets Ceres and Pallas, with the Elements of the Orbit of the former.*

Seeberg Obſervatory, near Gotha, May 31, 1802.

MOST HONOURED SIR,

HAVING proſecuted Dr. Olber's *Pallas* from April 4 till May 11, in the meridian, Dr. Gauſs, upon this ſet of my obſervations, calculated the elements of an eliptical orbit of this very remarkable heavenly body, which repreſent, with great accuracy, all the Seeberg obſervations.

Orbit of Pallas very eccentric, and probably linked in that of Ceres.

It appears, in general, by theſe calculations, that *Pallas is a planetary heavenly body, that moves between the orbits of Mars and Jupiter, with a very great eccentricity and inclination, and whoſe orbit comes very near to the orbit of the planet Ceres, perhaps touches it, perhaps even cuts it, like two links of a chain, this way* ∞, which cannot yet be aſſerted with certainty, the obſerved arc run over by this planet being too ſmall. Notwithſtanding, it appears already that the diſtances of Pallas and Ceres, in the line of nodes of their orbits, is very nearly equal. In the deſcending node, the diſtance of Pallas from the Sun is $= 2{,}86$, and the ſame diſtance of Ceres $= 2{,}93$. In the aſcending node, theſe diſtances are of greater inequality. Another very remarkable circumſtance is, that the mean motions of *Pallas* and *Ceres* are very nearly, perhaps abſolutely the ſame; though this cannot yet be aſſerted, becauſe the error of the obſervations of both planets is ſtill too great. But as far as yet appears, theſe mean motions will not differ very much; and in this caſe, ſmall as the maſſes of Ceres and Pallas may be, they will nevertheleſs exert a very ſenſible action one upon the other, and therefore give occaſion to very curious and intereſting inveſtigations in the mechanics of the heavens. The new planet *Pallas* will alſo call forth the utmoſt exertion of our analytical powers. Hitherto the two elements of a planetary orbit, *viz.* the eccentricity and the inclination, had been conſidered as an infinite little quantity, and ſo it might be, as theſe

Intereſting remarks on the ſingularity of the orbit of Pallas.

o elements, in all our old planets, are very fmall, fo that the higher powers of them could be neglected without danger, in calculating their mutual action, as they produced no fenfible term in the approximating feries. But this is no longer the cafe with Pallas, in which the eccentricity of the orbit and the inclination are fo very great.

Elements of Pallas.

Elements of the Orbit of the Planet Pallas, calculated by Dr. Gauß.

Epocha, March 31, at noon, in Seeberg,	161° 12′ 43,″2
Aphelium - - -	300 5 4, 0
Node - - - -	172 34 35, 0
Inclination - - -	35 0 42, 0
Mean daily heliocentric and tropical motion	757″ 166
Logarithm of half the greater axis -	0,4472636
Eccentricity - - - -	0,2591096

By thefe elements the whole feries of my obfervations are reprefented.

Computed and obferved places.

TABLE.

1802.	R.A. by Calculation.	Differenc.	Decl. by Calculat.	Difference.
Apr. 4	183° 44′ 5,″7	— 0,″9	13° 54′ 55,″4	+ 3,″4
5	183 34 24, 6	+ 0, 9	14 13 22, 6	— 0, 3
7	183 15 39, 6	+ 1, 6	14 49 1, 3	— 0, 7
8	183 6 35, 4	— 2, 4	15 6 15, 2	+ 5, 2
15	182 10 17, 9	+ 1, 4	16 54 35, 7	+ 4, 9
18	181 50 29, 4	— 1, 2	17 34 27, 1	———
19	181 44 30, 7	+ 5, 4	17 46 51, 9	— 2, 5
24	181 19 35, 8	— 2, 0	18 42 27, 9	———
25	181 15 38, 3	+ 6, 1	18 52 18, 9	———
26	181 12 2, 0	+ 0, 2	19 1 44, 9	— 4, 5
27	181 8 46, 6	+ 1, 0	19 10 46, 3	— 0, 5
29	181 3 21, 2	+ 4, 6	19 27 36, 2	— 7, 5
30	181 1 11, 4	+ 1, 5	19 35 24, 9	— 8, 6
May 1	180 59 22, 9	+ 0, 4	19 42 50, 8	+10, 0
2	180 57 56, 7	+ 4, 8	19 49 53, 2	+ 1, 6
3	180 56 52, 4	— 3, 2	19 56 32, 9	+14, 8
5	180 55 49, 6	— 5, 0	20 8 45, 6	+ 6, 9
6	180 55 51, 4	— 2, 7	20 14 19, 4	———
7	180 56 14, 9	— 8, 9	20 19 32, 1	— 3, 2
8	180 57 0, 4	— 9, 6	20 24 23, 7	+ 3, 6
11	181 1 25, 5	—16, 2	20 36 58, 8	—14, 4

The differences applied with contrary fign to the calculated A. R. and Declin. will give the obferved A. R. and Decl.

Pallas

Pallas and Ceres are now too near to the ſun, and the twilight permits no meridian obſervations. But aſtronomers who are provided with equatorials of great perfection, as, for inſtance, thoſe of Greenwich, Oxford, Richmond, and of Sir George Shuckburg, will be able to follow theſe two planets a longer time. The obſervation of Pallas will chiefly be of a very great value, as the ſeries of meridian obſervations is not for above five weeks. If more obſervations are not procured, it will be with ſome difficulty we ſhall find Pallas again next year; for the elements of an orbit calculated upon ſo ſmall an arc as $7\frac{1}{2}°$, may give an error of ſeveral degrees in January 1803. You will do, moſt honoured Sir, a great benefit to ſcience in general, and to aſtronomy in particular, if you engage the Engliſh aſtronomers, who have ſo very excellent and fixed equatorial ſectors, to follow Pallas out of the meridian as far as they can. For this purpoſe, I take the liberty to ſend you here an ephemeris of this planet's motion, calculated by Mr. Gauſs, which will enable aſtronomers to find it, and purſue their obſervations.

Theſe planets are now in the twilight.

Ephemeris of the Poſition of Pallas for Midnight, in Seeberg Obſervatory.

Ephemeris of Pallas.

1802.	R. Aſcenſ.	Declin.
May 24	181° 57′	21° 1′ N.
27	182 18	21 0
30	182 41	20 57
June 2	183 6	20 52
5	183 34	20 46
8	184 5	20 38
11	184 37	20 28
14	185 12	20 17
17	185 48	20 5
20	186 27	19 52
23	187 7	19 37
26	187 49	19 22
29	188 32	19 6

I am, with the greateſt eſteem and regard,

MOST HONOURED SIR,

Your moſt humble and obedient ſervant,

FRANCIS BARON DE ZACH.

XVII. *Method*

XVII.

of applying a temporary Forcer to a Pump, so as to pro- a constant Stream. By Mr. RICHARD TREVITHICK *. *From the Author.*

forcer ... on ...ducing ...nt THIS contrivance which, on several occasions, may prove useful, consists in fixing a barrel with a solid piston alongside of the common pump, in such a manner, that the lower space of the additional barrel may communicate with the space between the two valves of the pump, and lastly, by connecting ...iption. the rods so that they may work together. This is shewn in fig. 4, plate XI. and the effect is, that when the pistons are raised, the spaces beneath, A and B, become filled by the pressure of the atmosphere, at the same time that the upper column flows out at E. But again, when the pistons descend, the valve C shuts, and, consequently, the water driven by the piston in B must ascend through A, and continue to produce an equal discharge through E in the down stroke.

XVIII.

Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By EDWARD HOWARD, *Esq. F. R. S. From the Philosophical Transactions,* 1802.

Stony and metalline substances have undoubtedly fallen on the earth. THE concordance of a variety of facts seems to render it most indisputable, that certain stony and metalline substances have, at different periods fallen on the earth. Whence their origin, or whence they came, is yet, in my judgment, involved in complete obscurity.

The accounts of these peculiar Substances, in the early annals, even of the Royal Society, have unfortunately been blended with relations which we now consider as fabulous; and the more ancient histories of stones fallen from heaven,

* This Gentleman's name was, by mistake, printed Trevithack, in Vol. I. 161.

from

from Jupiter, or from the clouds, have evidently confounded fuch fubftances with what have been termed *Ceraunia*, *Bœtilia*, *Ombria*, *Brontia*, &c. names altogether unappropriate to fubftances fallen on our globe. Indeed fome miflead, and others are inexpreffive.

Why thefe facts have been difcredited.

The term Ceraunia, by a mifnomer, deduced from its fuppofed origin, feems, as well as Bœtilia *, to have been anciently ufed to denote many fpecies of ftones, which were polifhed and fhaped into various forms, though moftly wedgelike or triangular, fometimes as inftruments, fometimes as oracles, and fometimes as deities. The import of the names, Ombria, Brontia, &c. feems fubject to the fame uncertainty.

In very early ages, it was believed, that ftones did in reality, fall, as it was faid, from heaven, or from the gods; thefe, either from ignorance, or perhaps from fuperftitious views, were confounded with other ftones, which, by their compact aggregation, were better calculated to be fhaped into different inftruments, and to which it was convenient to attach a fpecies of myfterious veneration. In modern days, becaufe explofion and report have generally accompanied the defcent of fuch fubftances, the name of thunderbolt, or thunderftone, has ignorantly attached itfelf to them; and, becaufe a variety of fubftances accidentally prefent, near buildings and trees ftruck with lightening, have, with the fame ignorance, been collected as thunderbolts, the thunderbolt and the fallen metalline fubftance have been ranked in the fame clafs of abfurdity. Certainly, fince the phenomena of lightening and electricity have been fo well identified, the idea of a thunderbolt is ridiculous. But the exiftence of peculiar fubftances fallen on the earth, I cannot hefitate to affert: and on the concordance of remote and authenticated facts, I fhall reft the affertion.

Thunderbolt.

Mr. King, the learned author of *Remarks concerning Stones faid to have fallen from the Clouds, in thefe Days and in ancient Times*, has adduced quotations of the greateft antiquity, defcriptive of the defcent of fallen ftones; and, could it be thought neceffary to add antique teftimonies to thofe inftanced by fo profound an antiquarian, the quotations of Mons. Falconet, in his papers upon Bœtilia, inferted in the *Hiftoire des Infcriptions et Belles-Lettres*;† the quotations in Zahn's

Ancient authorities of ftones fallen on the earth. *King Falconet*, &c.

* Mercati, Metallotheca Vaticana. page 241..

† Tom. VI. P. 519. et Tom. XXIII. P. 228.

fpecula

Specula Phyſico-mathematica Hiſtoriana;* the *Fiſica Sotterranea* of Giacinto Gemma; the works of Pliny, and others might be referred to.

Dr. Chaldni on the Siberian Iron &c.

Doctor Chaldni, in his *Obſervations on the Maſs of Iron found in Siberia, and on other Maſſes of the like Kind*, as well as in his *Obſervations on Fire-balls and hard Bodies fallen from the Atmoſphere*, has collected almoſt every modern inſtance of phenomena of this nature.

M. Southey a ſtone wt. 10lbs 1796.

Mr. SOUTHEY relates an account, juridically authenticated, of a ſtone weighing 10 lb. which was heard to fall in Portugal, Feb 19, 1796, and was taken, ſtill warm, from the ground.†

Abbé Bachelay 1768.

The firſt of theſe peculiar ſubſtances with which chemiſtry has interfered, was the ſtone preſented by the Abbé Bachelay to the Royal French Academy. It was found on the 13th of September, 1768, yet hot, by perſons who ſaw it fall. It is deſcribed as follows:

"La ſubſtance de cette pierre eſt d'un gris cendré pâle; "lorſqu'on en regarde le grain à la loupe, on apperçoit que "cette pierre eſt parſemée d'une infinité de petits points bril- "lans métalliques, d'un jaune pâle; ſa ſurface extérieure, "celle qui, ſuivant M. l'Abbé Bachelay, n'étoit point en- "gagée dans la terre, étoit couverte d'une petite couche très- "mince d'une matière noire, bourſoufflée dans des endroits, "et qui paroiſſoit avoir été fondue. Cette pierre, frappée "dans l'intérieur avec l'acier, ne donnoit aucune étincelle; "ſi on frappoit, au contraire, ſur la petite couche extèrieure, "qui paroiſſoit avoir été attaquée par le feu, on parvenoit "à en tirer quelques-unes."

The ſpecific gravity of this ſtone was as 3535 to 1000.

Analyzed.

The academicians analyzed the ſtone and found it to contain.

Sulphur - - - - -	$8\frac{1}{2}$
Iron - - - - -	36
Vitrifiable earth - - -	$55\frac{1}{2}$
	100.

* Fol. 1696, Vol. I. p. 385. where a long enumeration of ſtones fallen from the ſky is given.

† Letters written during a ſhort reſidence in Spain and Portugal, p. 239.

Of

Of their mode of analyfis, I fhall have occafion to fpeak hereafter. They were induced to conclude, that the ftone, prefented to the Academy by the Abbé Bachelay, did not own its origin to thunder: that it did not fall from heaven; that it was not formed by mineral fubftances fufed by lightning: and that it was nothing but a fpecies of pyrites, without peculiarity, except as to the hepatic fmell difengaged from it by marine acid. " Que cette pierre, qui peut-être étoit " couverte d'une petite couche de terre ou de gazon, aura " été frappée par la foudre, et qu'elle aura été ainfi mife en " évidence: la chaleur aura été affez grande pour fondre la " fuperficie de la partie frappée, mais elle n'aura pas été affez " long-tems continuée pour pouvoir pénétrer dans l'intérieure " c'eft ce qui fait que la pierre n'a point été décompofée. " La quantité de matiééres métalliques qu'elle contenoit, en " oppofant moins de réfiftance qu'un autre corps au courant " de matiére électrique, aura peut-être pu contribuer même à " déterminer la direction de la foudre."

Conjecture that it might have been pyrites, ftruck in preference by lightning.

The Memoir is however concluded, by obferving it to be fufficiently fingular, that M. Morand le Fils had prefented a fragment of a ftone, from the environs of Coutances, alfo faid to have fallen from heaven, which only differed from that of the Abbé Bachelay, becaufe it did not exhale the hepatic fmell with fpirit of falt. Yet the academicians did not think any conclufion could be drawn from this refemblance, unlefs that the lightening had fallen by preference on pyritical matter.*

Another ftone from Coutances,

Mons. Barthold, Profeffeur à l'Ecole centrale du Haut-Rhin, gave I believe the next, and laft, † analytical account of what he alfo denominates *Pierre de Tonnerre*. He defcribes it thus: " La maffe de pierre connue fous le nom de Pierre de " Tonnerre d'Enfisheim, pefant environ deux quintaux, a la " forme extérieure arrondie, prefque ovale, raboteufe, d'un " afpect terne et terreux.

Barthold's examination of a ftone called thunder ftone weighing two quintals.

" Le fond de la pierre eft d'une couleur grife bleuâtre, " parfemée de criftaux de pyrites, ifolés, d'une criftalifation " confufe, en quelques endroits écailleufes, ramaffés, formant

Defcription.

* See Journal de Phyfique, Tom. II. p. 251.

† A very interefting detail of a meteor, and of ftones fallen in July 1790, was given by Profeffeur Baudin, in the *Magazin für das Neuefte aus der Phyfik*, by Profeffor Voigt.

" des

des nœuds et des petites veines, qui le parcourent en tout
" sens : la couleur des pyrites est doré ; le poli leur donné un
éclat d'acier, et, exposées à l'atmosphere, elles ternissent
" et brunissent. On distingue de plus, à l'œil nud, de la mine
" de fer grise, écailleuse, non sulfureuse, attirable à l'aimant,
" dissoluble dans les acids, peu oxidé, ou s'approchant beau-
" coup de l'état métallique.

Fracture. " La cassure est irrégulière, grenue, d'un grain un peu
" serré : dans l'intérieur on voit de très petites fentes. Elle ne
" fait pas feu au briquet : sa contexture est si lâche qu'elle se
" laisse entamer au couteau. En la pilant, elle se réduit assez
" facilement en une poudre grise bleuâtre d'une odeur terreuse.
" Quelquefois il se trouve de petits cristaux de mine de fer,
" qui resistent plus aux coups du pilon."

The specific gravity of the piece in Professor Barthold's possession, was 3233, distilled water being taken at 1000.

Composition. The analysis of Mons. Barthold, of which I shall also have occasion to speak hereafter, gave in the 100.

Sulphur - - - - -	2
Iron - - - - -	20
Magnesia - - - - -	14
Alumina - - - - -	17
Lime - - - - -	2
Silica - - - - -	42
	97

From the external characters, and from his analysis, the Professor considers the stone of Ensisheim to be argillo-ferruginous; and is of opinion that ignorance and superstition have attributed to it a miraculous existence, at variance with the first notions of natural philosophy.*

Sir W. Hamilton's account of stones that fell during a thunder storm. The account next in succession is already printed in the Transactions of the Royal Society; but cannot be omitted, as it immediately relates to one of the substances I have examined. I allude to the letter received by Sir William Hamilton, from the Earl of Bristol, dated from Sienna, July 12th, 1794. " In the midst of a most violent thunder-storm, " about a dozen stones, of various weights and dimensions, " fell at the feet of different persons, men, women, and " children. The stones are of a quality not found in any part

* See Journal de Physique. Ventose, An. 8, p. 169.

of

"of the Siennefe territory; they fell about eighteen hours "after the enormous eruption of Mount Vefuvius: which "circumftance leaves a choice of difficulties in the folution "of this extraordinary phenomenon. Either thefe ftones "have been generated in this igneous mafs of clouds, which "produced fuch unufual thunder; or, which is equally incre-"dible, they were thrown from Vefuvius, at a diftance of at "leaft 250 miles; judge then of its parabola. The philofo-"phers here incline to the firft folution. I wifh much, Sir, "to know your fentiments. My firft objection was to the "fact itfelf; but of this there are fo many eye witneffes, it "feems impoffible to withftand their evidence." (Phil. Trans. for 1795. p. 103.) Sir William Hamilton, it feems, alfo received a piece of one of the largeft ftones, which weighed upwards of five pounds; and had feen another, which weighed about one. He likewife obferved, that the outfide of every ftone which had been found, and had been afcertained to have fallen from the clouds near Sienna, was evidently frefhly vitrified, and was black, having every fign of having paffed through an extreme heat; the infide was of a light grey colour, mixed with black fpots and fome fhining particles, which the learned there had decided to be pyrites. Weight.

(To be continued.)

SCIENTIFIC NEWS, &c.

Dimenfions and Nature of the New Planets Ceres and Pallas. By Dr. HERSCHEL.

DR. HERSCHEL's obfervations on the two lately dif-covered celeftial bodies were read before the Royal Society, 6th of May.

Dr. Herfchel begins with ftating the refult of his attempts to meafure the diameter of the ftars difcovered by Piazzi and Olbers. He employed the lucid difc micrometer, which con-fifts of an illuminated circle viewed with one eye, while the other compares with it the magnified image formed by the te-lefcope; and he concludes, that the apparent diameter of Ceres was 22″, and of Pallas 17″ or 15″, at the diftance of nearly 1.634, and 1.187 from the earth refpectively, whence the apparent diameters at the diftance of the earth from the fun would be .35″ and .21″ or .16″ refpectively, and that their Obfervation of the diameters of Ceres and Pallas.

real

real diameters are about 163 and 95 or 71 English miles. here is no probability that either of these stars can have a tellite. The colour of Ceres is more ruddy than that of Pallas. They have generally more or less of a haziness, or coma, but sometimes, when the air is clear, this nebulosity scarcely exceeds the scattered light surrounding a very small star. From a view of all these circumstances, Dr. Herschel proceeds to consider the nature of the new stars. He thinks that they differ from the general character of planets, in their diminutive dimensions, in the great inclination of their orbits, in the coma surrounding them, and in the mutual proximity of their orbits; that they differ from comets in the want of eccentricity, and of a considerable nebulosity. Dr. Herschel therefore, wishes to call them asteroids, a term which he defines as a celestial body, which moves round the sun in an orbit either little or considerably eccentric, of which the plane may be inclined to the ecliptic in any angle whatever, the motion being either direct or retrograde, and the body being surrounded or not by a considerable atmosphere or a very small coma. This definition is intended to include such other bodies of the same kind as, Dr. Herschel supposes, will, in all probability, be hereafter discovered. Some additional observations show, that the apparent comas surrounding Ceres and Pallas, scarcely exceed those which are caused by aberration round the images of minute fixed stars.

J. of the Royal Institution.

Extract of a Letter from the Rev. James Wilson, D. D. Minister of Falkirk.

Falkirk, Stirlingshire, June 18th, 1802.

Durability of silk buried in the earth.

A few weeks ago the sexton of this parish, upon opening a grave in the church yard, found a ribband wrapped about the bone of an arm, which upon being washed was found to be entire, and to have suffered no injury, though it had lain for more than eight years in the earth; and had been in contact with a body which had passed through a state of corruption, and was reduced to its kindred dust. By what means did the silk resist the putrefactive process? it is not a compact substance like hair, horn, or bone, which are frequently found in graves after every other substance is completely changed. As silk is deprived of the gummy matter by the act of cleaning and scouring, and as this seems to be the chief animal substance

ftance which it contains, may not the remaining fibrous part be the better prepared to withftand the power of putrefaction? Accurate experiments tending to illuftrate this enquiry might be both amufing and inftructive.

ACCOUNT OF BOOKS OF SCIENCE.

Memoirs of the Literary and Philofophical Society of Manchefter, Vol. V. Part II. Octavo, 700 *Pages, with* 9 *Plates.* Cadell and Davies, London, 1802.

THIS publication of the refpectable Society of Manchefter is no lefs interefting than the former volumes of which the fcientific world well knows the value; it contains the following memoirs:—1. On tragedy, and the intereft in tragical reprefentations: An Effay. By the Rev. George Walker, F. R. S. and Profeffor of Theology in the new College, Manchefter.—2. Experiments and obfervations to determine whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers and raifed by evaporation; with an inquiry into the origin of fprings. By Mr. John Dalton.—3. Experiments and obfervations on the power of fluids to conduct heat; with reference to Count Rumford's feventh effay on the fame fubject. By Mr. John Dalton.—4. Experiments on the velocity of air iffuing out of a veffel in different circumftances; with the defcription of an inftrument to meafure the force of the blaft in bellows, &c. By Mr. Banks, Lecturer in Natural Philofophy. Communicated by Mr. Dalton.—5. Effay on the beautiful in the human form; and enquiry whether the Grecian ftatues prefent the moft perfect beauty of form that we at prefent have any acquaintance with. Communicated to the Society from a Correfpondent, through the Rev. George Walker.—6. A defence of learning and the arts, againft fome charges of Rouffeau: In two effays. By the Rev. George Walker, F. R. S.—7. Obfervations on the nervous fyftems of different animals; on original defects in the nervous fyftem of the human fpecies, and their influence on fenfation and voluntary motion. By John Hill, M. D.—8. Experiments and obfervations on the heat and cold produced by the mechanical condenfation and rarefaction of air. By

Mr. John Dalton.—9. Account of fome antiques lately found in the river Ribble. By Mr. Thomas Barritt.—10. Experimental effays on the conftitution of mixed gafes; on the force of fteam or vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; on evaporation; and on the expanfion of gafes by heat. By Mr. John Dalton.—11. A review of fome experiments, which have been fuppofed to difprove the materiality of heat. By Mr. William Henry.—12. An inveftigation of the method whereby men judge, by the ear, of the pofition of fonorous bodies relative to their own perfons. By Mr. John Gough. Communicated by Dr. Holme. 13.—On the theory of compound founds. By Mr. John Gough. Communicated by Dr. Holme. —14. Meteorological obfervations, made at Manchefter. By Mr. John Dalton.—Appendix, I. Explanation of a Roman infcription, found in Caftle-field, Manchefter. By Mr. Thomas Barritt. With a note on the fame fubject, by Dr. Holme.—II. Note to Mr. W. Henry's paper on heat.

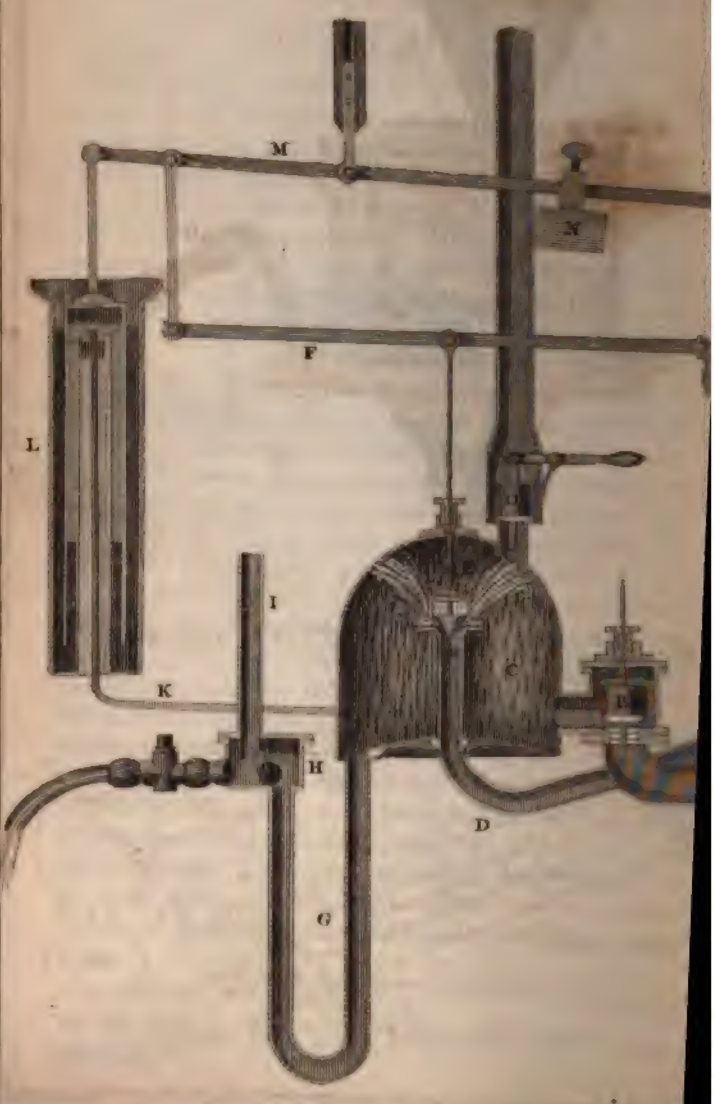
THE Rev. Thomas Falconer, A. M. of Bath, propofes to print by fubfcription, the Geography of Strabo, in feventeen books: tranflated from the Greek text; illuftrated by maps, coins, infcriptions, &c. accompanied with the notes of the older editors, and of the later; thofe of Thomas Falconer, Efq. of Chefter, the Oxford editor, entire; of Siebenkees, and Tzfchucke, of Germany; and thofe of the tranflator.

The conditions are—1. The work will be printed in a handfome manner, with foot notes: 2. It will be contained in three volumes quarto, if poffible: 3. The price will depend upon the Rate of paper when the work fhall be put to prefs; but it is hoped that four guineas will be the largeft eftimate: 4. Two guineas to be paid at the time of fubfcribing, for which a receipt fhall be given, and the remainder when half the work is delivered to fubfcribers: 5. The work will not be fent to prefs till three hundred copies are engaged, and only five hundred will be printed. Subfcriptions received by Meffrs. Cruttwell, and Bull, Bath; Cooke, Hanwell, and Parker, Oxford; Cadell and Davies, London; and Manners and Millar, Edinburgh.

ERRATA.

In Mr. Chenevix's paper, p. 114, for *fulphurated* read every where *fulphuretted*.

Mr. Woolf's Apparatus
for heating Water by Waste Steam.



Mill by Mr Garnet Terry

Fig. 1.

Drawback Lock by Mr Bullock.

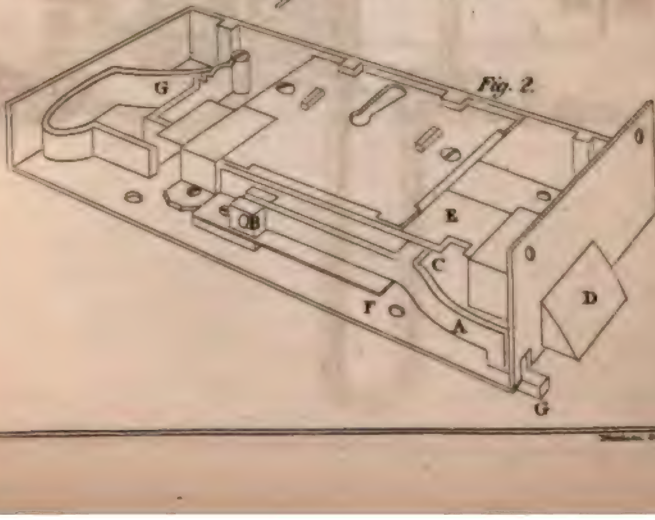
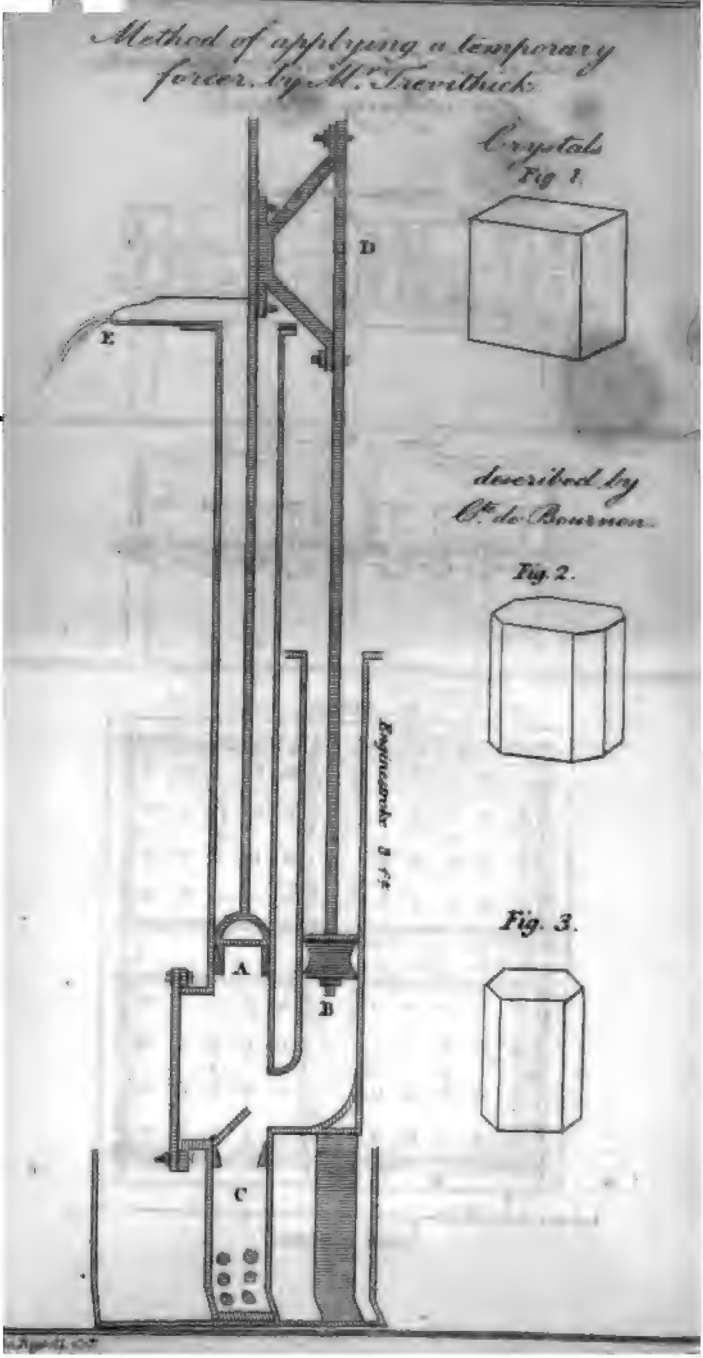
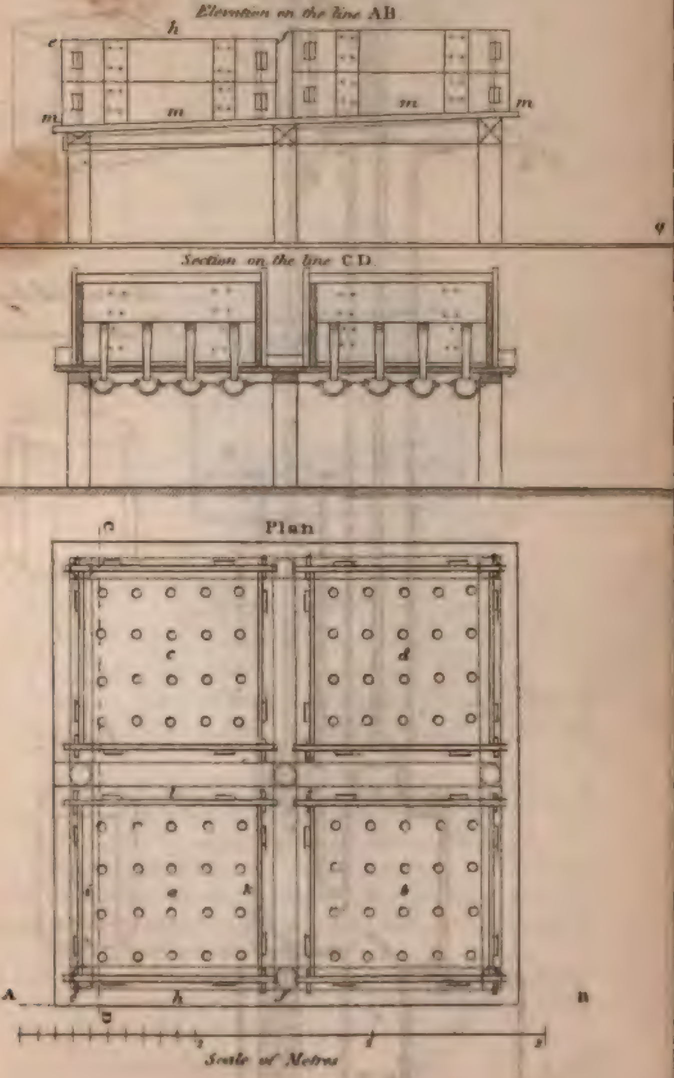


Fig. 2.


Method of applying a temporary forcer, by Mr. Trevithick.
D
E
Engine stroke 3 ft.
A
B
C
Crystals
Fig. 1.
described by
Ct. de Bournon.
Fig. 2.
Fig. 3.

Cit. Hapel Lachenaie's apparatus for claying Sugars.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1802

ARTICLE I.

On Granite. By Mr. ROBERT JAMESON. *Communicated by the Author.*

Sheriff Bræ; Leith, July 10, 1802.

Primitive rocks formed during the *chaotic* ftate by depofition from an aqueous folvent.

THE primitive rocks, of which granite is the oldeft, were formed during that period which Werner terms the *chaotic*, when the earth was ftill covered to a great height with water, and before organization had commenced. Their ftructure fhews that they have been depofited from a ftate of chemical folution *, and the diminifhing level of the newer ftrata, that

* To the idea of all foffil fubftances having been in a ftate of chemical folution in water, it has been objected, that many of them are intirely infoluble in water. To this, without adducing any of the numerous geological proofs, I anfwer, that we know not the original ftate of the different earthy and metallic fubftances; the artificial means we employ to procure them, may, and certainly has altered many of them from their original ftate.---J.

It may alfo be remarked, that many infoluble compounds are depofited by the chemical action of bodies which were foluble before they met. Our earths may be fuch compounds.---N.

the water has funk gradually and calmly. They ufually occupy the higher parts of the globe, but when covered with newer ftrata may form the loweft. The rocks which Werner confiders as belonging to this great clafs are, granite, gneifs, mica flate, primitive flate, porphyry, and fienite.

Granite.

As granite is in many refpects one of the moft important of thefe formations, I fhall here detail feveral interefting particulars refpecting it.

On the name granite.

Pliny, and the older writers, defcribe this rock under different names; the term granite appears to be of modern date, as Montfaucon is the firft writer who ufes it. This will not furprize us, when we confider, that it was not until the revival of letters when the remains of antiquity began to be ftudied, that the different rocks received particular denominations. To Werner we are indebted for the moft exact defcription; before his time it was confounded with fienite and grunftone, two rocks that differ both in their oryctognoftical and geognoftical characters.

Granite is an aggregate, granular, primitive rock, which is compofed of felfpar, quartz, and mica.

Aggregate component parts of granite; felfpar, quartz, and mica.

Felfpar is generally the prevailing, and mica the leaft confiderable ingredient. The felfpar has a confiderable range of colour; the principal colours are white, grey, red, and fometimes, though rarely, green: it is found in all the intermediate ftates, from very great to very fine grain. The quartz and mica are generally grey, and the firft has fometimes a black colour. Several foffils occur in granite befides thofe we have juft mentioned; thefe are fhorl and garnet: fuch varieties have received particular names, but the geognoft views them as accidental, and does not take particular notice of them. The topaz, which is diftinguifhed from all other precious ftones (excepting the emerald and garnet) by its occurrence in primitive mountains, is found accompanied with apatite in the tin beds which lie in granite at Ehrenfredersdorf.

Structure. Porphyritic.

Its *ftructure* is not fubject to much variety. When cryftals of felfpar are immerfed in a bafis of fine grained granite, it conftitutes what is termed porphyritic granite. Of this there are fine examples near to Carlfbad in Bohemia, and in many places in the north of Scotland. It fometimes occurs in glo-

Globular.

bular

bular diſtinct concretions *, and theſe are again compoſed of concentric lamellar, diſtinct concretions. This ſtructure of granite is only to be diſcovered, after the ſofter granite has weathered out; then theſe concretions which are vaſtly harder, and are only ſeparated from each other by the looſer granite, make their appearance. Upon the road between Dreſden and Bautzen I obſerved many fine examples of this ſtructure of granite: Mr. Barraud, in his intereſting deſcription of the Cape of Good Hope, mentions ſeveral globular diſtinct concretions of immenſe ſize. In Scotland the iſland of Arran affords inſtances of this kind †.

Diſtinctly ſtratified.

It is frequently obſerved diſtinctly ſtratified: in other inſtances owing to the thickneſs of the ſtrata, this ſtructure is difficultly obſervable, and has given riſe to the opinion that ſuch granite is not ſtratified. The Rieſengebirge, which ſeparate Sileſia from Bohemia, are for 150 miles compoſed of granite, diſpoſed in horizontal ſtrata. Laſt ſummer I examined theſe mountains along with a conſummate mineralogiſt, Dr. Mitchell, and we convinced ourſelves of the truth of this obſervation. I have obſerved ſimilar ſtratification in Saxony and Luſatia.

It ſeldom contains extraneous beds.

It is an intereſting fact in the natural hiſtory of granite, that it ſeldom contains extraneous beds, and Werner remarks, that the frequency of ſuch beds increaſes with the newneſs of the formation: thus gneiſs contains fewer beds than mica ſlate, and mica ſlate fewer than primitive ſlate.

No limeſtone.

Limeſtone, which accompanies all the newer primitive formations, is intirely wanting in granite.

Metals which occur in Granite.

Metals which occur in granite.

This rock is not ſo rich in metals and their ores, as the primitive rocks of newer formation. It contains, however, a conſiderable variety, and ſome of theſe have been as yet only diſcovered in granite. Iron, which is remarkable on account of its occurrence in every period of the earth's formation, is found interſperſed in the oldeſt granite. Red iron ore occurs

Iron.

* Theſe globular diſtinct concretions uſed formerly to be conſidered as bowlded ſtones, and afforded an invaluable opportunity for the framing of extravagant hypotheſis.

† Mineralogy of the Scottiſh Iſles, vol. i. p. 42.

in veins in granite, alfo the brown iron ore, but this is far feldomer than the red. Molybdena has as yet only occurred in granite, either interfperfed, or in veins of the oldeft formation, as at Schlackenwalde, Geya, and Altenberg.

Molybdena hitherto found only in granite.

Bifmuth, cobalt, blende, galena, and particularly tin.

Bifmuth, cobalt, blende, galena, and feveral ores of copper have been found in granite. Of all metals, however, tin is the one moft frequently found in granite, and in the great mining field of Cornwall, it is obferved, that copper occurs frequently in primitive flate, but the tin in granite.

The preceding obfervations refer principally to the old granite, which, as far as our experience goes, is the oldeft of all the rock formations. Werner has difcovered other granite formations, which are of a newer date.

More modern granite, alternating with gneifs.

1ft. Upon the Schneekoppe, the moft elevated part of the Riefengebirge, which is about 5000 feet above the level of the fea, granite alternates with gneifs, and hence Werner confiders it a diftinct formation *.

containing flate.

2d. At Greifenftein in Upper Saxony, Werner obferved granite which contained pieces of flate lying over ftrata of primitive flate, hence he juftly reckons it to be a diftinct formation, which is newer than either of the preceding.

Veins of granite traverfing ftrata of mica flate, and primitive flate.

3d. At Auerfberg, near Eibenftock in Saxony, and at Faftenberg near Johangeorgenftald, Werner difcovered veins of granite traverfing ftrata of mica flate and primitive flate, and this he is at prefent inclined to confider a new formation. In Scotland granite veins are very common, and feveral circumftances lead me to believe, that thefe and the Auerfberg are the fame formation. They are probably both connected with the Greifenftein formation. Werner mentions a few particulars, which he confiders as characteriftical for the newer granite formations.

Characters of the newer granite; ufually low, red, fine grained, &c.

1. Granite, which occurs in low fituations, may be fufpected to belong to the Greifenftein formation.

2. The newer granite has generally a deep red colour, is more frequently fine than coarfe grained, contains garnets, and is not porphyritic.

* I was fo fortunate as to have the opportunity of examining this formation in company with Dr. Mitchell. We obferved the granite alternations three times with the gneifs.

The

Mill by Mr. Garnet Terry.

Fig. 1.

Drawback Lock by Mr. Bullock.

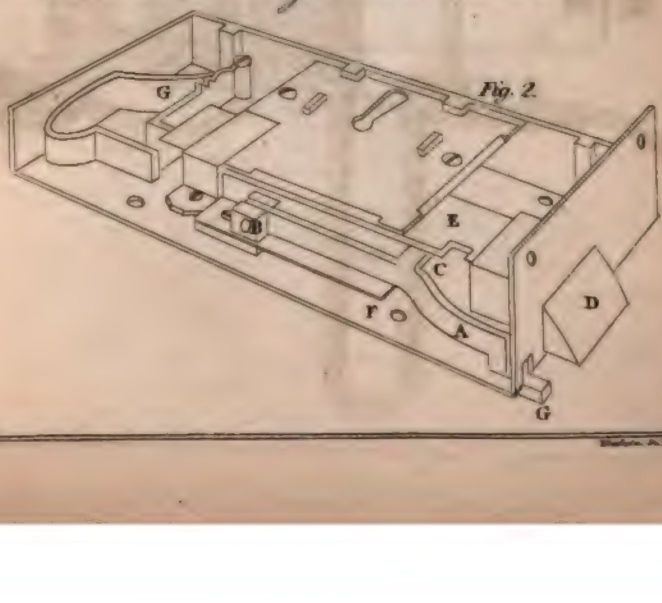


Fig. 2.

the line of feparation between a mafs of granite and the fchif-tus incumbent upon it, all around a tract of country, about eleven miles by feven, extending from the banks of Loch Ken weftward; and in all this tract they found, "that wherever the junction of the granite with the fchiftus was vifible, veins of the former, from fifty yards to the tenth of an inch in width, were to be feen running into the latter, and pervading it in all directions, fo as to put it beyond all doubt, that the granite of thefe veins, and confequently of the great body itfelf, which was obferved to form with the veins one uninterrupted mafs, muft have flowed in a foft and liquid ftate into its prefent pofition *. I have only further to add, that fome of thefe veins are remarkable for containing granite, not fenfibly different, in any refpect, from the mafs from which they proceed." The Criffle in Galloway, which is one of the moft confiderable portions of what Dr. Hutton confiders as the granite of that country, I found to be fienite, confequently it has no relation to the old granite formation. Profeffor Playfair, who examined the appearances at Loch Ken, believes with Dr. Hutton, that fienite and granite, in a geognoftic point of view, are to be confidered as the fame †. From this I draw the conclufion, that at Lock Ken we have a portion of the fame fienite as that which forms the Criffle.

which Mr. Playfair concludes to have flowed in a foft ftate to their prefent fituation.

The author obferved the Criffle to be fienite,

and infers that the other granites are a portion of the fame.

It appears then evident, that wherever granite, in the form of veins is to be obferved iffuing from granite into the contiguous ftrata of gneifs, mica flate, &c. it muft belong to a newer formation, and probably to that of Greifenftein. Many of the inftances where fuch appearances have been obferved, certainly belong to the fienite formation.

The fame inference generally ftated,

It is therefore demonftrated, that Granite is the oldeft Rock with which we are acquainted.

and that granite is the oldeft of rocks.

Before I conclude thefe remarks, I fhall notice two objections which have been urged againft the poffibility of granite veins having been filled from above. Mr. Playfair obferves at page 313, "that a ftrong objection to the fuppofed origin of granitic veins from infiltration, and indeed to their formation in any way but by igneous fufion, arifes from the number of

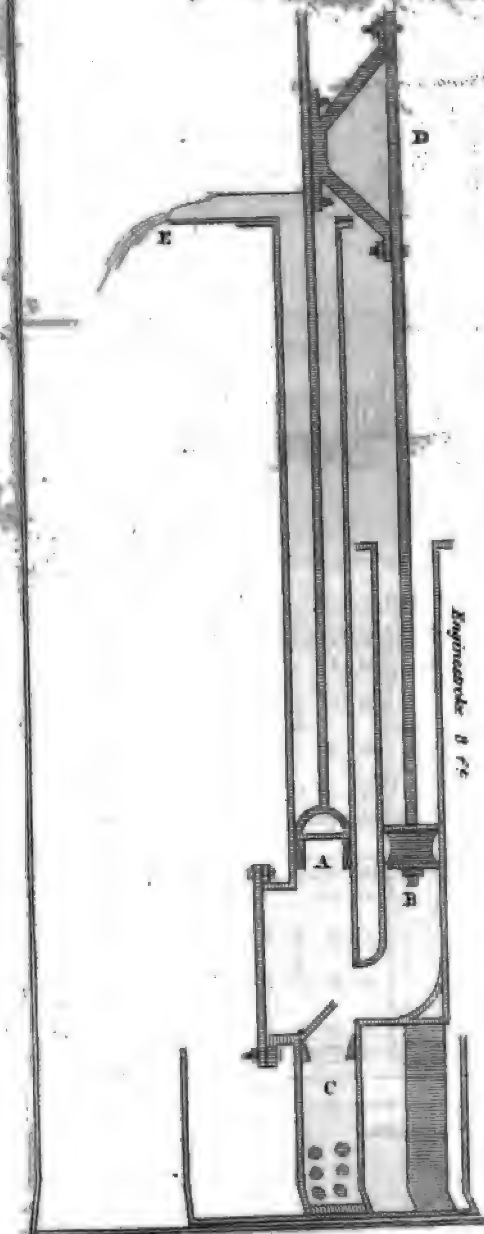
To Profeffor Playfair's remark that fragments of fchiftus could be infulated in granite veins only by igneous fufion;

* Tranfactions of the Royal Society of Edinburgh, vol. iii. p. 8.

† Illuftration of the Huttonian Theory, p. 312.

fragments

Method of applying a temporary forcer, by Mr. Trevithick.



Crystals

described by

Ct. de Bournon.

Fig. 1.

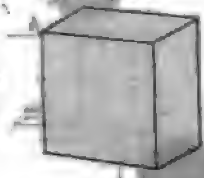


Fig. 2.

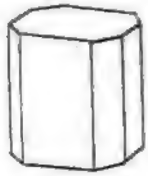
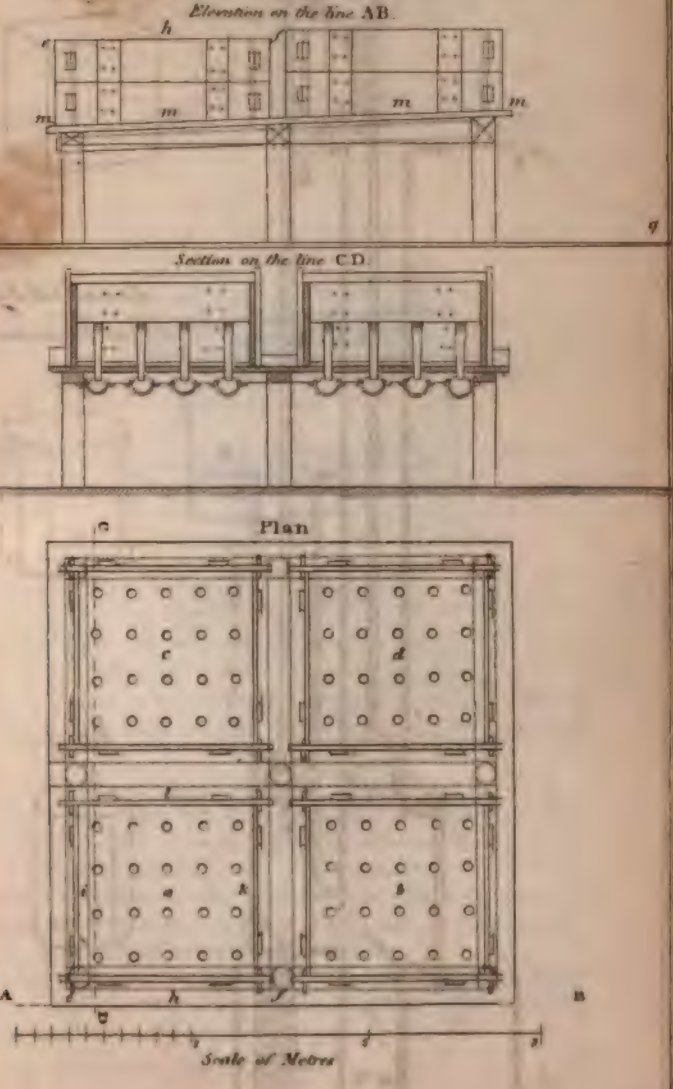


Fig. 3.

Cit. Hapel Lachenaie's apparatus for claying Sugars.



which they imbibed and became bright.

measures of it. In consequence of this, from being of a dark colour, they became exceedingly bright; and I concluded that they were now become *steel*, though I was not able to ascertain it by a direct experiment.

These left in vitriolic acid a black matter as steel does.

But after dissolving those bright filings in diluted vitriolic acid, a quantity of black matter, as after the solution of steel, remained unaffected by it. This being heated in inflammable air imbibed a considerable quantity of it, and then, by means of diluted vitriolic acid, gave inflammable air very copiously.

Its habitudes were those of finery cinder.

This black matter had evidently the properties of finery cinder.

The black matter from pure steel was affected in the same manner.

I then dissolved 200 grains of *broken watch springs*, which are undoubtedly pure steel, and collected from the solution three grains of black matter. Heating this in inflammable air, a great proportion of it was imbibed; and then, by means of the diluted acid, it gave out inflammable air as copiously as iron or steel filings would have done.

It was therefore finery cinder, and not carbon.

This black matter, therefore, from the solution of steel was finery cinder, and not carbon, or plumbago. And as iron acquires weight by becoming finery cinder, and this addition of weight, I think I have proved to be from *water*, it can hardly be doubted, but that the addition of weight to iron, in being converted into steel, is from the same cause. Indeed, I believe it to be impossible to expose iron to a red heat in circumstances in which there is any possible access of water, or of air, which always contains water, without a partial calcination of it; that is without its becoming superficially at least finery cinder.

Bright needles afforded much less black matter than watch springs, of which the surface was blued or oxided.

This was evidently the difference between the result of the solution of the *watch springs*, and that of an equal weight, viz. 200 grains of *broken polished needles*, which had not undergone any calcination. For the black matter that remained from the solution of them would not have weighed a quarter of a grain. Giving colour to steel, which is done to watch springs, is always a partial calcination of the metal; and this appears from the preceding experiments to be the conversion of a part of it into finery cinder, which is the reverse of plumbago; being, according to the new theory, an *oxide* in the highest degree; whereas if plumbago contain any oxigen, it is in the lowest degree.

III. An

III.

An Account of the Art of making Glue. In a Letter from Mr. JOHN CLENNEL.

DEAR SIR, *Newcastle, June* 21, 1802.

THE following attempt to develope the "art and myftery" of glue-making is at your fervice, if you think it worthy a place in your very valuable mifcellany. The improvement of any manufacture depends upon its eafy accefs to men of fcience, and a prudential theorift can never be better employed than in attempting to reduce to regularity or to fyftem the manufactures that may fall under his attention. In conformity to the firft principle, I made fome notes whilft vifiting a glue manufactory a few years ago in Southwark, and thofe, interwoven with the remarks on that fubject of fome chemifts of the firft refpectability, I take the liberty of fending you: at the fame time I muft beg of you, or your correfpondents, that where it may be corrected in any manner, it may be done, and I fhall feel myfelf obliged by the attention.

Great advantages of the vifitation and report of manufactories.

Glue is an infpiffated jelly, made of the parings of hides or horns of any kind, the pelts obtained from furriers, and the hoofs and ears of horfes, oxen, calves, fheep, &c. quantities of all which are imported in addition to the home fupply, by many of the great manufacturers of this article: thefe are firft digefted in lime water, to cleanfe them as far as it can from the greafe or dirt they may have contracted; they are then fteepped in clean water, taking care to ftir them well from time to time; afterwards they are laid in a heap, and the fuperabundant water preffed out; then they are boiled in a large brafs caldron with clean water, fkimming off the dirt as it rifes, and further cleanfed by putting in, after the whole is diffolved, a little melted alum or lime finely powdered, which, by their deterfive properties, ftill further purge it: the fkimming is continued for fome time, when the mafs is ftrained through bafkets, and fuffered to fettle, that the remaining impurities, if any, may fubfide; it is then poured gently into the kettle again, and further evaporated by boiling a fecond time, and fkimming, until it becomes a clear but darkifh brown colour: when it is thought

Glue is made from refufe of hides, &c.

Cleaned from greafe by lime-water; then fteeped in clean water; drained; boiled in water; clarified by alum or lime; ftrained; cleared by fubfidence; again boiled to the requifite denfity; poured into moulds; cut into portions; and dried on a net.

thought to be ſtrong enough (which is known either by the length of time a certain quantity of water and materials have boiled, or by its appearance during ebullition), it is poured into frames or moulds of about ſix feet long, one broad, and two deep, where it hardens gradually as the heat decreaſes: out of theſe troughs or receivers it is cut when cold by a ſpade, into ſquare pieces or cakes, and each of theſe placed within a ſort of wooden box, open in three diviſions to the back; in this the glue, as yet ſoft, is taken to a table by women, where they divide it into three pieces * with an inſtrument not unlike a bow, having a braſs wire for its ſtring; with this they ſtand behind the box and cut by its openings, from front to back; the pieces thus cut are taken out into the open air, and dryed on a kind of coarſe net work, faſtened in moveable ſheds of about four feet ſquare, which are placed in rows in the glue-maker's field (every one of which contains four or five rows of net work); when perfectly dry and hard, it is fit for ſale.

haracter of
ood glue.

That is thought the beſt glue which ſwells conſiderably without melting, by three or four days immerſion in cold water, and recovers its former dimenſions and properties by drying. Glue that has got froſt, or that looks thick and black, may be melted over again and refined, with a ſufficient quantity added of freſh to overcome any injury it may have ſuſtained; but it is generally put into the kettle after what is in it has been purged in the ſecond boiling. To know good from bad glue, it is neceſſary for the purchaſer to hold it between his eye and the light, and if it appears of a ſtrong dark brown colour, and free from cloudy or black ſpots, the article is good.

udgement of
ie cakes.

I am, Sir,

With great reſpect,

Your's, &c.

JOHN CLENNELL.

* When the women, by miſtake, cut only two, that which is double the ſize is called a Biſhop, and thrown into the kettle again.

IV. On

IV.

On the Preparation of Indelible Ink. In a Letter from Mr. THOMAS SHELDRAKE.

To Mr. NICHOLSON.

SIR,

AS your correfpondent, Mr. Clofe, has alluded to my memoir on the nature and preparation of drying oils, it may not be unpleafant to receive fuch information as I am able to give refpecting the object of his purfuit.

Amber is not foluble in alcohol or effential oil.

By experiments repeated and varied every way that my imagination can fuggeft, I am convinced that amber is not foluble in alcohol or any effential oil: it is foluble in expreffed oils, by the procefs defcribed in Lewis's philofophical commerce of the arts, but that folution does not dry well, and therefore will not anfwer Mr. Clofe's purpofe; but when diffolved by the * well-known procefs for making amber-varnifh, it is likely to anfwer extremely well.

The folution in expreffed oil is not good for ink; but the folution for varnifh is good.

Afphaltum promifes to be a good material.

There is another fubftance which feems likely to anfwer his purpofe very well. † Afphaltum is a bituminous fubftance, perfectly black when viewed in a mafs, but a dark tranfparent brown when diffolved: it is foluble in fpirit of turpentine at a

Procefs for the oily folution of amber.

* The following is the moft convenient method: Put fmall pieces of amber into an iron ladle, fet it on a fire till they are melted, then add fo much of the beft drying oil as will make it liquid, ftir them well together, and, when cold, add fo much fpirit of turpentine as will make it thin enough to flow from the brufh. The object in making varnifh is to difcolour the amber as little as poffible; therefore it is but little roafted, and the lighteft coloured drying oil is ufed; but if this folution of amber was ufed for making ink, the darknefs of the colour would be an advantage, therefore the amber fhould be thoroughly melted, and the darkeft drying oils ufed in preference to the others.

Good afphaltum is now eafily procured.

† Within thefe few years good afphaltum may be procured in many fhops in London: before that period it was unknown; the beft of what *was fold, and is ftill fold in fome places,* was the caput mortuum of amber; other compofitions of pitch and various refins were likewife fold for afphaltum. If it were ufed for Mr. Clofe's ink, care fhould be taken to felect the beft.

low

low heat, and when diffolved runs freely from the pen: I have known fome artifts draw with this in preference to ink, becaufe its colour harmonizes better with other materials alfo ufed in drawing, and becaufe it is indelible, as it ftrikes immediately into the paper, and if it is not thick, will ftrike through it: by this means every ftroke made with it is vifible through every colour that is wafhed over it.

Afphaltum in fpirit of turpentine, rendered confiftent with folution of amber, and coloured with lamp black, would give an indelible ink.

It feems then that if a folution of afphaltum was made in fpirit of turpentine, and fo much of the folution of amber was added as would give it due confiftence, and the fineft lamp black to give it colour, a perfect ink would be formed, and poffeffing thofe properties Mr. Clofe feems to defire; for, fuppofing the other materials could be removed, fo much of the colour as depends on the afphaltum would be indelible, except by fuch means as would deftroy the paper or parchment.

The drying oil of the varnifh would increafe the difficulty of obliteration.

Even the fmall quantity of drying oil introduced into the varnifh, would be ufeful in this refpect; for it is well known, that if oil is dropped upon white paper, though the mark is fcarcely vifible at firft, in a year or two it will become a dark yellowifh brown: it feems as though the oil changes the paper fo much that its colour can never be recovered, at leaft thofe who undertake to reftore the white colour of old prints, always make an exception to fpots of oil.

If this hint fhould be thought deferving any notice, you will have the goodnefs to make what ufe you pleafe of it.

I am, Sir,

Your's, &c.

T. SHELDRAKE.

No. 50, Strand, July 6, 1802.

Copal would probably be difcharged from paper by camphorated fpirit.

P. S. I believe Mr. Clofe will find himfelf miftaken as to the infolubility of copal when ufed in his ink. So powerful is the influence of camphor upon it, that if copal be reduced to powder, and a little camphor is rubbed into it, it immediately begins to foften, and the whole foon becomes a coherent mafs; and though copal is not foluble in alcohol alone, if camphor is added, it diffolves as eafily in the compound as the fofteft refin would. It is therefore extremely probable, that, if a paper written with his ink was wafhed with camphorated fpirits, the writing would be removed with very little difficulty.

V. *Obfervations*

V.

Obſervations on the Cauſes why a large Quantity of common Salt prevents Putrefaction, and a ſmall Quantity haſtens it. By D. H.

To Mr. NICHOLSON.

SIR,

THERE are few phenomena of nature more intereſting, and at the ſame time more involved in obſcurity, than the two oppoſite actions of muriate of ſoda, which is known to have very conſiderable effect, both in accelerating and in retarding putrefaction. The *antiſeptic* property of this ſalt has been known from the earlieſt ages. It was diſcovered, however, by Pringle, Macbride, and Gardane, that putrefaction may be haſtened by ſprinkling the animal ſubſtances with water holding a ſmall quantity of muriate of ſoda in ſolution. This diſcovery excited much ſurpriſe; and the celebrated chemiſts who obſerved it ſeem to have been fully aware of the difficulties attending its explanation, as they have offered no theory to account for it. It would doubtleſs appear a preſumptuous undertaking, to attempt the ſolution of a queſtion which has baffled the ingenuity of ſo many philoſophers, did not the ſubſequent diſcoveries in chemiſtry and phyſiology enable us to ſpeculate on the ſubject with ſome degree of probability.

Remarkable oppoſition of effects of common ſalt in preventing or accelerating putrefaction; as its quantity is more or leſs.

It ſeems neceſſary for the decompoſition of an animal ſubſtance, *1ſt*, that it be in contact with atmoſpheric air; *2dly*, that it be expoſed to a moderate degree of heat; and, *3dly*, that it be impregnated with humidity. It muſt neceſſarily follow, that whatever removes theſe conditions will check the progreſs of putrefaction. Of this we have many inſtances, as in the effects of cold; in covering the ſubſtances with ſugar, reſins, &c. and in preſerving them in ſpirit of wine. I conceive with Gren, that muriate of ſoda acts only in this way, by abſtracting the moiſture, and removing the ſubſtance from the contact of oxigen; and not by a peculiar innate, and as it were hidden *(vis occulta)* antiſeptic power *.

Putrefaction requires air, warmth, and moiſture;

and is checked by cold and covering from the air.

Crude common ſalt ſuppoſed to preſerve by drying and covering the body it is applied to.

* Gren's Chemiſtry, Chap. VIII.

With

Putrefaction is hastened by destroying irritability.

With regard to the *septic* property of muriate of soda, it must be referred to another cause. The destruction of muscular irritability appears to be a chief cause of accelerating putrefaction. This was ascertained by Mr. John Hunter, who found, that when death is occasioned by an electric shock, by violent exercise, by a blow on the stomach, or by any thing that destroys the irritability of the muscular fibre, putrefaction quickly ensues. Fontana found that the same effects were produced by the poison of vipers. It has been also found by experiment, that the compounds of potash and soda destroy muscular irritability. Now, is it not a fair inference from these premises, that a small quantity of muriate of soda should possess a septic quality? Upon these grounds, it will not be difficult to reconcile the two opposite actions of muriate of soda. When a large quantity of this salt is applied to an animal substance, it acts merely by removing the indispensible conditions of putrefaction, air and moisture. The particles in contact with the substance may indeed act in destroying the irritability of the muscular fibre; but this being only a secondary cause of putrefaction, cannot operate unless in conjunction with air and moisture. On the other hand, when a small quantity of this salt in solution is applied, it is insufficient either to exclude the air, or to abstract the moisture; its peculiar property, therefore, acts *in conjunction* with the other causes, and these causes united accelerate the putrefactive process much more than any of them separately.

Common salt inferred to produce that effect.

Much salt retards putrefaction by drying and covering, more than it accelerates by the last mentioned quality.

Little salt accelerates it for the contrary reason.

Such is the explanation of these phenomena which occurred to me. Although it may be imperfect in many points, yet it appears to involve no hypothesis, but to be a strict deduction from facts. Should this attempt have the good fortune to meet with your approbation, its insertion in the Philosophical Journal may at least have the effect of drawing the attention of some more eminent chemist to this too much neglected subject.

I am, Sir,

Your's respectfully,

Edinburgh, July 12, 1802. D. H.

VI. *Account*

VI.

Account of the Methods by which Soda is at present prepared for the English Market; with other Observations. By Mr. FRED. ACCUM. *From the Author.*

Soda obtained by decompofing the fulphate of the bleachers with American potafh.

SINCE the late new duty on falt, the manufacturers of foda avail themfelves of the method of decompofing the fulphate of foda *, by means of what is called American potafh. Though this kind of potafh contains lefs pure alkali than the Ruffian or German potafh, it is imported in a ftate of perfect drynefs, whereas the other is always met with in a moift ftate; fo that the abfence of water appears to compenfate for the deficiency of the alkali. The American potafh fells now from 48s. to 54s. per cwt. it has been confiderably higher, until lately it fluctuated between 54s. and 56s.

Particular defcription of the manufacturing procefs.

I have been employed in a foda manufactory in which the following method anfwered exceedingly well. Five hundred pounds of fulphate of foda were introduced into an iron boiler containing a fufficient quantity of Thames water. 560 lb. of American potafh were likewife diffolved in as little water as poffible in an iron boiler fixed near the former. The potafh was always previoufly tried, and if indifferent the quantity taken was 10 lb. more.

A boiling folution of potafh was added to another of fulphate of foda,

then boiled,

The folution, as near as I recollect, was made with about thirty pails of water to the alkali here mentioned. Both folutions were then made to boil, and as foon as the ebullition took place the folution of potafh was ladled into the boiler containing the fulphate of foda. The mixture was agitated during the transfufion, and the fire raifed as expeditioufly as poffible. As foon as the fluid boiled it was ladled into a wooden gutter, which conveyed it into a ciftern of wood lined with fheet lead nearly half an inch thick, which was fixed in a cool place. Sticks of wood were then placed acrofs the ciftern, from which flips of fheet lead two or three inches wide were hung into the fluid at four inches diftant from each other.

* Sulphate of foda is fold cheap by the bleachers, who have it as the refidue of decompofing common falt by fulphuric acid with manganefe. In February, 1801, it was worth from 11s. to 14s. per cwt. but I have not the prefent price.---A.

and drawn off to cryſtallize in a leaden ciſtern. other. When all was cool, which in the winter was generally the caſe in three days, a plug in the bottom of the ciſtern was drawn in order to let off the fluid, and the cryſtallized ſalt was taken from the ſlips of lead. The bottom exhibited a rock of ſalt, which was detached by chiſſel and mallet. On this account it is that the lead which lines the ciſtern muſt be thick, in order to guard againſt accidents. For if the metal be perforated, the ſaline ſolution creeps between it and the wood, and in a very ſhort time detaches the lining, and it is beſides extremely difficult to find out the place where the defect really is. *at a temperature under 55°.* The temperature where the ſoda is left to cryſtallize ought not to exceed 55° Fah. *The ſoda is then waſhed with cold water;* In this ſtage of the proceſs the whole of the ſalt is waſhed in the ſame ciſtern with cold water, to clear it of impurities; after which it is transferred again into the boiler, *then diſſolved and evaporated at a low heat.* diſſolved in clear water, and evaporated by heat. As ſoon as a ſtrong pellicle is formed, it is ſuffered to cool ſo far that the hand may be dipped in the fluid without injury, and the heat is kept at that temperature *Pellicles of ſulphate of potaſh are formed and fall down;* as long as effectual pellicles continue to be formed over the whole ſurface of the boiler, and then fall to the bottom. When no more pellicles are formed, or at leaſt only by blowing with the mouth upon the ſurface, the fire is withdrawn, and the fluid is ladled out into the ciſtern to cryſtallize. The ſulphate of potaſh, &c. which had been depoſited, is then *which ceaſing, the fluid is drawn off to cryſtallize.* taken out of the boiler and put aſide. If the fluid be ſuffered to cool pretty low before it is ſuffered to run into the ciſtern, very little ſulphate of potaſh is found in the ſoda; but in general the rocky maſſes of ſoda met with in the market contain *100 parts of ſulphate of ſoda afford 138 of carbonate.* a conſiderable quantity. By this proceſs from 136 to 139 lb. of ſoda may be obtained from 100 lb. of ſulphate of ſoda, if the ſoda be cryſtallized in large cryſtals; if ſmall cryſtallized it yields leſs; it ſells now at 52s. or 54s. per cwt. and is retailed at 8d. per lb.

The two cryſtallizations are found to be neceſſary. We might be inclined to ſuppoſe that the firſt operation was unneceſſary, and that the ſoda might be ſeparated at once from the ſulphate of potaſh at the inſtant of its formation; but practice will convince the operator otherwiſe. A conſiderable loſs is manifeſted if the proceſs be not conducted in this manner; though the diſcovery of the cauſe may perhaps be not ſo eaſily accompliſhed as the proof of the fact.

Other

Another procefs. The acid of fulphate of foda is decompofed and expelled by heat with charcoal, and the foda extracted by water, &c.

Other manufacturers grind together 500 cwt. of Glauber's falt of the bleachers, and 100 cwt. of charcoal; they expofe this mixture in a reverberatory furnace refembling a baking oven, till the matter when ftirred with a rake becomes pafty. It is then withdrawn and transferred into large cafks, each provided with a double bottom. Water is then fuffered to ftand one inch high over it for 24 hours; the cock is then opened, the folution runs through the perforated bottom, over which a ftratum of ftraw had been previoufly placed; and is thence conducted into the boiler for evaporation and cryftallization *.

The heat muft be applied on an iron, and not a brick hearth.

It is a curious fact, that iron plates are abfolutely neceffary to conftitute the furface on which thefe articles are expofed to heat: fire bricks do not anfwer. It feems as if the iron affifted the union; though neither iron filings mixed with the articles nor pyrites are found of advantage.

Uncertainty of this method.

This method of making foda is extremely uncertain. If the heat be not raifed gradually, or if the mixture be not fufed enough, or a little too much, it does not fucceed. The worft event is, that when the mixture has been made too hot, fulphuric acid is produced, and fulphate of potafh is formed.

It is faid to be profitable if well managed.

The quantity of foda which may be obtained by this procefs, is faid to be equal to that obtained by any other method.

Third method. Sulphate of foda decompofed by acetite of lime, of which the acid is had by diftilling wood.

I have been lately informed, that in Germany foda is made by decompofing the fulphate of foda by means of acetite of lime; the acetic acid is obtained for that purpofe from wood, and the charcoal is found to pay the cofts.

The method recommended by acetite of lead, or elfe oxides of that metal is productive of lofs in this country. The muriate of lead thus obtained is not a pigment.

The method recommended by feveral chemifts, of obtaining foda by decompofing Glauber's falt by acetite, or the oxides of lead, does not anfwer in this country. The mafs is by far too bulky; and requires too much time, attendance, and fuel to reduce it to a narrow compafs. I have been informed by men well fkilled in this department, that it is nearly impracticable in the large way. The muriate of lead which is produced cannot be ufed as a white pigment, as the inventors pretend.

Pruffian method by quicklime; then flaking and long expofure to the air. Product fulphate of lime and carbonate of foda.

In Pruffia they mix muriate of foda and quick lime together; then flake the lime, and form the whole into a thick pulp,

* This was the procefs at the falt works.---N.

 which

which is extended about two inches thick over a large furface, and left in that fituation for three months. Carbonate of foda is then formed, which is wafhed out and cryftallized in the ufual manner. The foda obtained by this procefs always has a yellowifh caft.

VII.

Comparifon of the French definitive Metre with an Englifh Standard, brought from London by M. A. PICTET, *one of the Editors of the Bibliothéque Britannique**.

Short hiftory of admeafurements of the earth.

THE meafurement of the earth, and the inveftigation of its figure, were the fubjects, at various times in the courfe of the eighteenth century, of the labours of a number of philofophers of the firft eminence in different countries. Some Swedifh aftronomers are now employed in a fecond meafurement of the fame degree which was meafured fixty years ago by the French Academicians in Lapland, under the polar circle. In France, when the idea of feeking in the dimenfions of the globe itfelf the unit to which all meafures and weights might be referred, had once been conceived and adopted, it was neceffary to make an effort proportional to the importance of an undertaking which was thus become national. In the midft of a long and fanguinary war, together with difficulties of every other kind, a chain of triangles has been formed between Dunkirk and Barcelona, comprehending the tenth part of the arc of the meridian which extends from the Equator to the pole, and which is equal to one fourth of the circumference of the globe; and the ten millionth part of this arc, thus determined, has been adopted for the unit of the metrical fyftem: it has been fixed by the conftruction of ftandards made of fubftances proper to refift the attacks of time; and by a careful examination of the precife relation of the length of the metre to that of the pendulum vibrating feconds, on the level of the fea, in a given latitude, the determination of this unit has been rendered independent of any accident that might deftroy or

particularly that lately made in France.

and the ftandard meafure thence deduced.

* From No. 148 of the Bibliotheque Britanique. I avail myfelf of the free tranflation given in the Journal of the Royal Inftitution; but have very carefully read the proof with the original.---N.

4 impair

impair the ftandards reprefenting it; while in the formation of thefe ftandards all the precautions have been employed that could be fuggefted by the prefent improved ftate of natural philofophy, and of the arts.

In England, on the other hand, operations have been carried on for thefe five and twenty years, which are to be the foundations of an exact map of Great Britain. Thefe labours, begun by the late General Roy, have been conducted with much fagacity and precifion; and the refults are likely to procure very interefting information refpecting the figure of the earth. Sir George Shuckburgh, an eminent member of the Royal Society of London, has fuccefsfully employed himfelf in private, in refearches intended to fix the precife length of the ftandards, which have ferved as bafes for the meafurements made in Great Britain. Operations of the fame kind in England.

It was therefore to be regretted, that operations fo fimilar, conducted in two neighbouring countries, and capable of acquiring a new intereft by comparifon, fhould remain unconnected, for want of an actual ftandard of the meafures of the one country, which might be tranfported into the other, after the definitive determination of the French meafure. This regret we had deeply felt at various times when thefe objects were laid before our readers; and we may fay wtth truth, that if the hope of procuring this medium of comparifon was not the only motive of the journey to England that one of us has made, it at leaft greatly contributed to induce him to undertake it. Great advantage of an accurate comparifon of the French and Englifh ftandards.

Our colleague took fome fteps in his paffage through Paris, to obtain an authentic metre, in order to be fubmitted to the examination of the Royal Society, to which he has the honour of belonging, but he did not remain long enough in Paris to be able to fucceed in this attempt. He took advantage of his longer ftay in England, in procuring from the hands of Mr. Troughton, an artift celebrated for his accuracy in the conftruction and divifion of geometrical and aftronomical inftruments, a ftandard rigoroufly conformable to that which he had made for Sir George Shuckburgh, and with which this philofopher had compared the principal Englifh ftandards. Our colleague procured alfo from the fame artift the comparative apparatus of Sir George Shuckburgh, compofed of two excellent microfcopes, the one bearing a micrometer which divides Means ufed by M. Pictet to obtain a true Britifh ftandard. Apparatus by *Troughton* for comparing linear meafures.

vides the Englifh inch into ten thoufand equal parts. Upon his return to Paris he made hafte to exhibit thefe inftruments to the Minifter of the Interior, and to the National Inftitute. This learned body nominated three of its members, in order to proceed to the regular comparifon of the definitive metre with the Englifh ftandard. The undertaking, by no means fo eafy as it at firft appeared, occupied the committee in five different meetings, of nearly four hours each; and it was performed with all the care and precaution that the nature of the fubject required. Mr. Prony, who, as the tranflator of General Roy's memoir on the firft trigonometrical operations in England, was particularly interefted in thefe refearches, acted as fecretary to the Committee, and it was at his houfe, and with the affiftance of a comparative apparatus belonging to him, that the principal experiments were made. He has been fo obliging as to furnifh us with an authentic copy of the report made to the Inftitute, which was deemed of fufficient confequence to be read at the public fitting of the laft quarter. He adds, that "This report will foon be followed by a memoir, in which he will enter into more circumftantial details of all the obfervations that he has made; and in which he will give a defcription and a figure of his comparative inftrument." We fhall bear in mind this promife, and in the mean time we fhall give our readers a copy of the report; informing them that we have beftowed on the correction of the proofs of this important paper all the attention neceffary to enable us to affirm that no typographical error has been committed in the numbers.

Commiffion of the French National Inftitute for comparing.

National Inftitute of Sciences and Arts. 6 *Nivofe, Year* 10 *(27th December,* 1801.*)*

A member read, in the name of a committee, the following report on the comparifon of the ftandard metre of the Inftitute with the Englifh foot.

Report of the Inftitute.

M. Pictet, Profeffor of Natural Philofophy at Geneva, fubmitted to the infpection of the clafs in the month of Vendémiaire, an interefting collection of objects relative to the fciences and arts, which he collected in his journey to England.

Among them was a ftandard of the Englifh linear meafure, engraved on a fcale of brafs, of 49 inches in length, divided by very fine and clear lines into tenths of an inch.

Englifh ftandard of 49 inches,

It

It was made for M. Pictet by Troughton, an artist in London, who has deservedly the reputation of dividing instruments with singular accuracy; it was compared with another standard made by the same person for Sir George Shuckburgh, and it was found that the difference between the two was not greater than the difference between the divisions of each; that is, it was a quantity absolutely insensible. This standard may therefore be considered as identical with the standard described by Sir George Shuckburgh in the Philosophical Transactions for 1798. made by Troughton.

M. Pictet also exhibited to the Institute a comparer, or an instrument for ascertaining minute differences between measures, constructed also by Mr. Troughton. It consists of two microscopes with cross wires, placed in a vertical situation, the surface of the scale being horizontal, and fixed at proper distances upon a metallic rod. One of them remains stationary at one end of the scale, the other is occasionally fixed near to the other end; and its cross wires are moveable by means of a screw, describing in its revolution $\frac{1}{100}$ of an inch, and furnished with a circular index, dividing each turn into 100 parts; so that having two lengths which differ only one tenth of an inch from each other, we may determine their difference in ten thousandths of an inch. The wires are placed obliquely with respect to the scale, so that the line of division must bisect the acute angle that they form, in order to coincide with their intersection. General Roy has described, in the 75th volume of the Philosophical Transactions, a similar instrument made by Ramsden, for measuring the expansion of metals. Instrument of comparison bearing two microscopes.

M. Pictet offered to the class the use of the standard, with the micrometer described, for the determination of the comparative length of the metre, and the English foot: the offer was accepted with gratitude, and M M. Legendre, Méchain, and Prony, were appointed to co-operate with M. Pictet in the comparison of the standard metre of platina and the English foot.

The first meeting was on the 28th Vendémiaire (21st of October,) at the house of Mr. Lenoir. First meeting of the commission.

At first a difficulty occurred from the different manner in which the measures were defined: the English scale was graduated by lines; the French standards were simply formed to the length of a metre: hence the length of the metre could not easily Difficulty to compare the English scale graduated by lines, and the French, which is of the precise

length without graduation.

eafily be taken by the microfcopes; nor could the Englifh fcale be meafured by the method employed for making new ftandard metres, which confifts in fixing one end againft a firm fupport, and bringing the other into contact with the face of a cock or flider, adjufted fo as barely to admit the original ftandard between it and the fixed furface.

Method by an intermediate piece.

Mr. Lenoir attempted to overcome this difficulty by reducing to a thin edge the terminations of a piece of brafs of the length of a metre; fo that it was compared with the ftandard metre in the ufual manner, and its extremities, when placed on the Englifh fcale, conftituted two lines parallel to thofe which were really engraved on the fcale, and capable of being viewed by the microfcopes.

Experimental comparifon by this method.

The ftandard metre of platina, and another ftandard of iron, belonging alfo to the Inftitute, were thus compared with the Englifh foot; each of thefe two meafures being equal, at the temperature of melting ice, to the ten millionth part of the quadrant of the meridian. At the temperature of 15.3° of the decimal thermometer, or 59.5° of Fahrenheit, the metre of platina was equal to 39.3775 Englifh inches; and that of iron to 39.3788, meafured on M. Pictet's fcale.

Refult,

in fome refpects uncertain.

Thefe firft experiments, fhowed, however, that the method employed was liable to fome uncertainty, arifing from the difficulty of placing the crofs wires precifely at the extremity of the thin edge of the plate of brafs employed in the comparifon; a reflection or irradiation of light, which took place at that extremity, prevented its being diftinctly obferved if the optical axis of the microfcope was precifely a tangent to the furface exactly at the termination.

Accurate method of comparifon by contacts; a ruler carrying a line was caufed to move through the precife length of the metre, under the microfcopes.

In order to remove this inconvenience, another arrangement was propofed by one of the Committee. (It was Mr. Prony that fuggefted this ingenious method, and M. Paul of Geneva, who happened to be prefent, that executed it. B.B.) A line was traced on a fmall metallic ruler, perpendicular to its length; the end of the ruler was fixed againft a folid obftacle, and the crofs wires made to coincide with the line: the ftandard metre was then interpofed between the fame obftacle and the end of the piece, and the line traced on it, which had now obvioufly advanced the length of the metre, was fubjected to the other microfcope. To the microfcopes thus

thus fixed, the graduated fcale was transferred; one of the divifions was placed exactly under one of the microfcopes, and the micrometer fcrew was turned in order to meafure the fraction, expreffing the diftance of the other microfcope from another divifion.

The comparifon was repeated in the fame manner the 4th Brumaire (26th October laft) at the houfe of one of the Committee, and after feveral experiments, agreeing very fatisfactorily with each other, it was found that at the temperature 12.75° centigrade, or 55° of Fahrenheit, the ftandard of platina was 39.3781, and that of iron 39.3795 Englifh inches. Precife refult at 55° of Fahrenheit.

The two metres being conftructed to be equal at the temperature of melting ice, thefe operations may be verified by reducing their refults to that temperature. For this determination we are provided with the accurate experiments made by Borda, and the committee of weights and meafures, on the dilatation of platina, brafs, and iron; from which it appears, that for every degree of the decimal thermometer, platina expands .00000856; iron .00001156; and brafs .00001783; (for Fahrenheit's fcale thefe quantities become 476, and 642, and 990 parts in an hundred millions.) From thefe data we find that, at the freezing point, the ftandard metre of platina was equal to 39.38280, and that of iron to 39,38265 Englifh inches of M. Pictet's fcale. The difference is lefs than the 500th of a line, or the 200000th of the whole metre, and is therefore wholly inconfiderable. Reduction to the temperature of melting ice.

The refult of the whole comparifon is therefore this. Suppofing all the meafures at the temperature of melting ice, each of the ftandard metres is equal to the 10000000th part of the quadrant of the meridian, and to 39.38272 Englifh inches of M. Pictet's fcale. Standard metre at 32° is = 39.38272 Englifh inches.

At the clafs of mathematical and phyfical fciences of the National Inftitute, 6 Nivofe, year 10.

Legendre, Méchain, and Prony, *Reporter*.

This report is approved, and its conclufions adopted by the clafs. Certified in conformity with the original by Delambre.

Paris, 26 Nivofe, year 10 (16th January, 1802).

[*The reft of this Paper is by the learned Secretary to the Royal Inftitution, Dr. Young.*]

On examining the reduction of the ftandards of platina and iron to the freezing point, it appears that they differ fomewhat lefs Revifion by Dr. Young.

lefs than is ftated in the report, and that they coincide within an unit in the laft place of the decimals expreffing their magnitudes, or one ten thoufandth of an inch. The ftandard of platina at the freezing point becomes equal to 39.37380, and that of iron to 39.37370 Englifh inches on the fcale of brafs at 55°, and the mean of thefe to 39.37100 Englifh inches at 62°, which is the temperature that has been univerfally employed in the comparifon of Britifh ftandards, and in the late trigonometrical operations in particular. This refult agrees furprifingly with Mr. Bird's determination of the lengths of the toifes fent by Mr. Lalande to Dr. Mafkelyne, of which the mean was 76.734 inches: hence the metre, having been found to contain 36.9413 French inches, appears to be equal to 39.3702 Englifh inches: or rather to be either 39.3694 or 39.3710, accordingly as the one or the other of the two toifes happens to have been the more correct; we may therefore give the preference to that which meafured 76.736 inches.

Allowing the accuracy of the French meafurements of the arc of the meridian, the whole circumference of the globe will be 24855.43 Englifh miles, and its mean diameter 7911.73. Taking the ellipticity at $\frac{1}{310}$, the axis will be nearly $7893\frac{1}{2}$, the equatorial diameter 7928, and the diameter of a fphere of equal folid content about 7916 miles; the brafs ftandard being at the temperature of 62° of Fahrenheit.

Standard metre at 62°.

As long, therefore, as the Englifh ftandard continues to be reduced to this temperature, we muft confider the metre as equivalent to 39.3710, and not to 39.3827 Englifh inches.

Upon thefe joint authorities it may be of ufe to reprint here a table of the principal meafures and weights now ufed in France, with the very flight corrections which this laft comparifon has introduced into it. In tranflating the French terms into Englifh, we are fully at liberty to refcue them, in fome meafure, from the barbarifms in orthography which have been committed in forming them.

Meafures of length, the metre being at 32°, the foot at 62°.

French meafures of length,

		English inches.
Millimetre	- - - - -	.03937
Centimetre	- - - - - -	.39371
Decimetre	- - - - -	3.93710

Metre

Metre - - - - -	39.37100
Decametre - - - -	393.71000
Hecatometre - - - -	3937.10000
Chiliometre - - - -	39371.00000
Myriometre - - - -	393710.00000

	M.	F.	Y.	Ft.	In.
A decametre is - - -	0	0	10	2	9.7
A hecatometre - - - -	0	0	109	1	1
A chiliometre - - - - -	0	4	213	1	10.2
A myriometre - - - -	6	1	156	0	6

8 chiliometres are nearly 5 miles.

Meaſures of capacity.

	Cubic inches E.
Millilitre - - - - - - -	.06103
Centilitre - - - - - -	.61028
Decilitre - - - - - - -	6.10280
Litre, a cubic decimetre - - - -	61.02800
Decalitre - - - - - -	610.28000
Hecatolitre - - - - - -	6102.80000
Chiliolitre - - - - - -	61028.00000
Myriolitre - - - - - -	610280.00000

of capacity.

A litre is nearly $2\frac{1}{8}$ wine pints. 14 decilitres are nearly 3 wine pints. A chiliolitre is 1 tun, 12.73 wine gallons.

Weights.

A gramme is the weight of a cubic centimetre of pure water at its maximum of denſity. It has been found equal to 18.827 French grains, of which 576 made 472.5 Engliſh; and 489.5058 grammes make a pound of the ſtandard of the mint at Paris. Weights.

	E. grains.
Milligramme - - - - - -	.0154
Centigramme - - - - - - -	.1544
Decigramme - - - - - -	1.5444
Gramme - - - - - - -	15.4440
Decagramme - - - - - -	154.4402
Hecatogramme - - - - -	1544.4023
Chiliogramme - - - - - -	15444.0234
Myriogramme - - - - -	154440.2344

A deca-

decagramme is 6 dwts. 10.44 gr. tr.; or dr. iiß. gr. 4.44 ; or 5.65 dr. avoird. A hecatogramme is 3 oz. 8.5. . A chiliogramme is 2lbs. 3 oz. 5 dr. av. A myriogramme is 22lbs. 1.15 oz. av. 100 myriogrammes are 1 ton nting 32.8lbs.

Agrarian meafures.

...n mea-	Are, 1 fquare decametre - - -	3,95 perches.
	Hecatare - - -	2 acres, 1 rood, 35.4 perches.

For fire-wood.

...-wood.	Decifiere, $\frac{1}{10}$ ftere - - -	3.5317 cub. f. E.
	Stere, 1 cubic metre - -	35.3171 cub. f.

	MONEY. *Copper.*	E. grains.
...ey.	Centime, 1 gramme - - - - -	15.4
	5 centimes, or fous - - - - -	77.2
	Decime - - - - - - -	154.4
	2 decimes - - - - - -	308.8

Silver $\frac{9}{10}$ or $\frac{18}{20}$ fine.

Franc, 5 grammes, - - - - -	3 dwts. 5.2 gr.
6 francs, - - - - - -	16 dwts. 2.1 gr.

The franc is nearly the fame with the livre tournois, and worth about 10d. Bolton's penny weighs 435 gr.; his halfpenny 165; a fhilling nearly 93 gr. and is $\frac{37}{40}$ fine.

Length of pendulum, &c. It appears from Mr. Borda's experiments, that in latitude 45°, a pendulum of the length of a metre would perform in a vacuum 86116.5 vibrations in a day: the length of a pendulum being fuppofed to increafe with the latitude, in the proportion of the fquare of the fine of the latitude, multiplied by .000567, while the time of its vibration remains unaltered.

Y.

VIII. On

VIII.

On the Figure of Sulphate of Barytes, and the Formation of Mandreporæ. In a Letter from Mr. H. SARJEANT.

To Mr. NICHOLSON.

SIR,

IF the following mineralogical notices appear to contain any thing worthy of attention, they are at your fervice, from

Your humble fervant,

Kefwick, July 16, 1802. H. SARJEANT.

THE barytes, or ponderous earth, occurs in a great variety of forms: one of the rareft is defcribed as "refembling a num-"ber of fmall double convex lenfes fet edgeways in a ground." This fingular formation, fo different from the angular forms affected by cryftals in general, was the caufe of my examining it with confiderable attention; and I am perfuaded, that whoever does the fame, will perceive that the fhape of thefe cryftals is not, properly fpeaking, lenticular, but rather a fort of very acute edged rhomboid, inferted by one of the corners as far as the diagonal line, fo that the projecting part refembles the corner of a carpenter's chiffel, admitting that the angle formed by its edge and fide were enlarged to 100 degrees, or fomewhat more, and the fide bevilled off in the fame manner as the edge, but in the oppofite direction.

In its moft ufual form, the fulphate, the barytes is a very abundant production. It is found in this and the neighbouring counties, in great quantities, with lead, with lime (common limeftone), with iron, and with gypfum.

That fort of limeftone which abounds with the petrefactions called mandreporæ, appears to owe its origin to depofitions of calcareous mud, formed in a manner fimilar to what may be feen in the upper part of moft lakes, that of Kefwick in particular, in which a fpecies of equifetum grows, often in fheets of feveral acres, flightly covered with water. The fection of the madreporæ is precifely that of the frefh root, and they are generally inclined all in the fame direction, as if caufed by a ftream of water paffing over them, which might be the cafe when the water was drawn off from fuch places, by the effect of earthquakes, or other great operations of nature.

The fubfequent induration of fuch mud into ftone, may be confidered as a fact fufficiently known.

IX. *Experiments*

IX.

Experiments and Obfervations on certain ftony and metalline Subftances, which at different Times are faid to have fallen on the Earth; alfo on various Kinds of native Iron. By EDWARD HOWARD, *Efq. F. R. S. From the Philofophical Tranfactions.* 1802.

(Continued from page 221.)

Stone that fell in Yorkfhire and was exhibited in London. 1796, weighing 56lb. Hiftorical account.

IN 1796, a ftone weighing 56lb. was exhibited in London, with feveral atteftations of perfons who, on the 13th of December, 1795, faw it fall, near Wold Cottage, in Yorkfhire, at about three o'clock in the afternoon. It had penetrated through 12 inches of foil and 6 inches of folid chalk rock; and in burying itfelf, had thrown up an immenfe quantity of earth, to a great diftance: as it fell, a number of explofions were heard, about as loud as piftols. In the adjacent villages, the founds heard were taken for guns at fea; but, at two adjoining villages, were fo diftinct of fomething fingular paffing through the air, towards the habitation of Mr. Topham, that five or fix people came up, to fee if any thing extraordinary had happened to his houfe or grounds. When the ftone was extracted, it was warm, fmoaked, and fmelt very ftrongly of fulphur. Its courfe, as far as could be collected from different accounts, was from the fouth-weft. The day was mild and hazy, a fort of weather very frequent in the Wold hills, when there are no winds or ftorms; but there was not any thunder or lightning the whole day. No fuch ftone is known in the country. There was no eruption in the earth; and, from its form, it could not come from any building; and, as the day was not tempeftuous, it did not feem probable that it could have been forced from any rocks, the neareft of which are thofe of Hamborough Head, at a diftance of twelve miles.* The neareft volcano, I believe to be Hecla, in Iceland.

Inveftigations refpecting it.

The exhibition of this ftone, as a fort of fhow, did not tend to accredit the account of its defcent, delivered in a hand-bill at the place of exhibition; much lefs could it contribute to re-

* Extracted from the printed paper delivered at the place of exhibition.

move

move the objections made to the fall of the ſtones preſented to the Royal French Academy. But the Right Hon. preſident of the Royal Society, ever alive to the intereſt and promotion of ſcience, obſerving the ſtone ſo exhibited to reſemble a ſtone ſent to him as one of thoſe fallen at Sienna, could not be miſled by prejudice; he obtained a piece of this extraordinary maſs, and collected many references to deſcriptions of ſimilar phenomena. At length, in 1799, an account of ſtones fallen in the Eaſt Indies was ſent to the preſident, by John Lloyd Williams, Eſq. which, by its unqueſtionable authenticity, and by the ſtriking reſemblance it bears to other accounts of fallen ſtones, muſt remove all prejudice. Mr. Williams has ſince drawn up the following more detailed narrative of facts.

Account of the Exploſion of a Meteor, near Benares, in the Eaſt Indies; and of the falling of ſome Stones at the ſame Time, about 14 Miles from that City. By John Lloyd Williams, Eſq. F. R. S.

Exploſion of a meteor near Benares, and the falling of ſome ſtones at the ſame time.

A circumſtance of ſo extraordinary a nature as the fall of ſtones from the heavens, could not fail to excite the wonder, and attract the attention of every inquiſitive mind.

Among a ſuperſtitious people, any preternatural appearance is viewed with ſilent awe and reverence; attributing the cauſes to the will of the Supreme Being, they do not preſume to judge the means by which they were produced, nor the purpoſes for which they were ordered; and we are naturally led to ſuſpect the influence of prejudice and ſuperſtition, in their deſcriptions of ſuch phenomena; my inquiries were therefore chiefly directed to the Europeans, who were but thinly diſperſed about that part of the country.

Narrative.

The information I obtained was, that on the 19th of December, 1798, about eight o'clock in the evening, a very luminous meteor was obſerved in the heavens, by the inhabitants of Benares and the parts adjacent, in the form of a large ball of fire; that it was accompanied by a loud noiſe, reſembling thunder; and that a number of ſtones were ſaid to have fallen from it, near Krakhut, a village on the north ſide of the river Goomty, about 14 miles from the city of Benares.

Large ball of fire with noiſe like thunder.

The meteor appeared in the weſtern part of the hemiſphere, and was but a ſhort time viſible: it was obſerved by ſeveral Europeans, as well as natives, in different parts of the country. In

In the neighbourhood of Juanpoor, about 12 miles from the ſpot where the ſtones are ſaid to have fallen, it was very diſtinctly obſerved by ſeveral European gentlemen and ladies who deſcribed it as a large ball of fire, accompanied with a loud rumbling noiſe, not unlike an ill diſcharged platoon of muſquetry. It was alſo ſeen, and the noiſe heard, by various perſons at Benares. Mr. Davis obſerved the light come into the room where he was, through a glaſs window, ſo ſtrongly as to project ſhadows, from the bars between the panes, on a dark coloured carpet, very diſtinctly; and it appeared to him as luminous as the brighteſt moonlight.

Degree of light, &c.

Inveſtigation on the ſpot by Mr. Davis.

When an account of the fall of the ſtones reached Benares, Mr. Davis, the judge and magiſtrate of the diſtrict, ſent an intelligent perſon to make inquiry on the ſpot. When the perſon arrived at the village near which the ſtones were ſaid to have fallen, the natives, in anſwer to his inquiries, told him, that they had either broken to pieces, or given away to the Teſſeldar (native collector) and others, all that they had picked up; but that he might eaſily find ſome in the adjacent fields, where they would be readily diſcovered, (the crops being then not above two or three inches above the ground,) by obſerving where the earth appeared recently turned up. Following theſe directions, he found four, which he brought to Mr. Davis: moſt of theſe, the force of the fall had buried, according to a meaſure he produced, about ſix inches deep, in fields which ſeemed to have been recently watered; and it appeared, from the man's deſcription, that they muſt have lain at the diſtance of about a hundred yards from each other.

Account by the natives.

What he further learnt from the inhabitants of the village, concerning the phenomenon, was, that about eight o'clock in the evening, when retired to their habitations, they obſerved a very bright light, proceeding as from the ſky, accompanied with a loud clap of thunder, which was immediately followed by the noiſe of heavy bodies falling in the vicinity. Uncertain whether ſome of their deities might not have been concerned in this occurrence, they did not venture out to enquire into it until the next morning; when the firſt circumſtance which attracted their attention was, the appearance of the earth being turned up in different parts of their fields, as before mentioned, where, on examining, they found the ſtones.

The

The affiftant to the collector of the diftrict, Mr. Erfkine, a very intelligent young gentleman, on feeing one of the ftones, brought to him by the native fuperintendant of the collections, was alfo induced to fend a perfon to that part of the country, to make inquiry; who returned with feveral of the ftones, and brought an account fimilar to that given by the perfon fent by Mr. Davis, together with a confirmation of it from the Cauzy, (who had been directed to make the inquiry) under his hand and feal. Inquiries by Mr. Erfkine.

Mr. Maclane, a gentleman who refided very near the village of Krakhut, gave me part of a ftone that had been brought to him the morning after the appearance of the phenomenon, by the watchman who was on duty at his houfe; this, he faid, had fallen through the top of his hut, which was clofe by, and buried itfelf feveral inches in the floor, which was of confolidated earth. The ftone muft, by his account, previous to its having been broken, have weighed upwards of two pounds. and Mr. Maclane.

At the time the meteor appeared, the fky was perfectly ferene; not the fmalleft veftige of a cloud had been feen fince the 11th of the month, nor were any obferved for many days after. State of the weather.

Of thefe ftones, I have feen eight, nearly perfect, befides parts of feveral others, which had been broken by the poffeffors, to diftribute among their friends. The form of the more perfect ones, appeared to be that of an irregular cube, rounded off at the edges; but the angles were to be obferved on moft of them. They were of various fizes, from about three to upwards of four inches in their largeft diameter; one of them, meafuring four inches and a quarter, weighed two pounds twelve ounces. In appearance, they were exactly fimilar: externally, they were covered with a hard black coat or incruftation, which in fome parts had the appearance of varnifh, or bitumen; and, on moft of them were fractures, which, from their being covered with a matter fimilar to that of the coat, feemed to have been made in the fall, by the ftones ftriking againft each other, and to have paffed through fome medium, probably an intenfe heat, previous to their reaching the earth. Internally, they confifted of a number of fmall fpherical bodies, of a flate colour, embedded in a whitifh gritty fubftance, interfperfed with bright fhining fpiculæ, of a metallic or pyritical nature. The fpherical bodies were much Stones of this defcription which have been feen by the author. Some account of the fame.

 harder

harder than the reſt of the ſtone: the white gritty part readily crumbled, on being rubbed with a hard body; and, on being broken, a quantity of it attached itſelf to the magnet, but more particularly the outſide coat or cruſt, which appeared almoſt wholly attractable by it.

The author declines any conjecture.

As two of the more perfect ſtones which I had obtained, as well as parts of ſome others, have been examined by ſeveral gentlemen well verſed in mineralogy and chemiſtry, I ſhall not attempt any further deſcription of their conſtituent parts; nor ſhall I offer any conjecture reſpecting the formation of ſuch ſingular productions, or even record thoſe which I have heard of others, but leave the world to draw their own inferences from the facts above related. I ſhall only obſerve, that it is well known there are no volcanos on the continent of India; and, as far as I can learn, no ſtones have been met with in the earth, in that part of the world, which bear the ſmalleſt reſemblance to thoſe above deſcribed.

Iron in the Bornian collection ſaid to have fallen in Bohemia.

IT remains for me to ſpeak of a ſubſtance mentioned in the *Lithophylacium Bornianum*, Part I. page 125, deſcribed thus: "Ferrum retractorium, granulis nitentibus, matrice vireſcenti "immixtis, (*Ferrum virens*, Linn.) cujus fragmenta, ab unius "ad vigenti uſque librarum pondus, cortice nigro ſcoriaceo "circumdata, ad Plann, prope Tabor, circuli Bechinenſis Bo-"hemiæ, paſſim reperiuntur."

The iron thus deſcribed, is moreover made remarkable by a note *, which obſerves, that credulous people aſſert it to have fallen from heaven during a thunder ſtorm, on the 3d of July, 1753.

The collection of Baron Born, it is well known, has a place in the cabinet of the Right Honourable Charles Greville, who, from the effect produced by comparing the hiſtories and ſtructure of the Italian and Yorkſhire ſtones with the deſcription of this iron, was induced to ſearch the collection of Born, where he diſcovered the very ſubſtance aſſerted to have fallen on the 3d of July, 1753. How far theſe four ſubſtances have reſemblance to each other, it will ſoon appear not to be my province to anticipate.

* Quæ (fragmenta) 3 Julii, anni 1753, inter tonitrua, e cœlo pluiſſe creduliores quidam aſſerunt.

The

The Prefident having done me the honour to fubmit his fpecimens of the Yorkfhire and Italian ftones to my examination, I became indebted to Mr. Greville and Mr. Williams for a fimilar diftinction: and, being thus poffeffed of four fubftances, to all of which the fame origin had been attributed, the neceffity of defcribing them mineralogically did not fail to prefent itfelf. To execute this tafk, no one could be more eager, and certainly no one better qualified, than the Count de Bournon. He has very obligingly favoured me with the following defcriptions.

Other ftones of fimilar origin.

Mineralogical Defcription of the various Stones faid to have fallen upon the Earth. By the Count de BOURNON, *F. R. S.*

THE ftones I am about to defcribe, are not of any regular fhape; and thofe which were found in an entire ftate, that is, thofe which had not been broken, either by their fall or otherwife, were entirely covered with a black cruft, the thicknefs of which was very inconfiderable.

Count Bournon's defcription of the various ftones.

The ftones which fell at Benares, are thofe of which the mineralogical characters are the moft ftriking: I fhall therefore begin the following defcription with them; and fhall afterwards make ufe of them, as objects of comparifon, in defcribing the others.

STONES FROM BENARES.

Thefe ftones, as well as the others defcribed in this paper, whatever may be their fize, are covered over the whole extent of their furface with a thin cruft of a deep black colour: they have not the fmalleft glofs; and their furface is fprinkled over with fmall afperities, which caufe it to feel, in fome meafure, like fhagreen, or fifh fkin.

Stones from Bernares. Thin black cruft; rough like fifh fkin.

When thefe ftones are broken, fo as to fhew their internal appearance, they are found to be of a greyifh afh colour; and of a granulated texture, very fimilar to that of a coarfe gritftone: they appear evidently to be compofed of four different fubftances, which may be eafily diftinguifhed, by making ufe of a lens.

Fracture; coarfe grained, grayifh afh colour; compofed of four fubftances; *viz.*

One of thefe fubftances, which is in great abundance, appears in the form of fmall bodies, fome of which are perfectly globular, others rather elongated or elliptical. They are of various fizes, from that of a fmall pin's head to that of a pea, or nearly fo:

Opake gray globules, of conchoidal fracture and flight luftre, flightly giving fire with fteel, and abrading glafs;

ſo: ſome of them, however, but very few, are of a larger ſize. The colour of theſe ſmall globules is gray, ſometimes inclining very much to brown: and they are completely opaque. They may, with great eaſe, be broken in all directions: their fracture is conchoid, and ſhews a fine, ſmooth, compact grain, having a ſmall degree of luſtre, reſembling in ſome meaſure that of enamel. Their hardneſs is ſuch, that, being rubbed upon glaſs, they act upon it in a ſlight degree; this action is ſufficient to take off its poliſh, but not to cut it: they give faint ſparks, when ſtruck with ſteel.

Pyrites, not magnetical.

Another of theſe ſubſtances, is a martial pyrites, of an indeterminate form: its colour is a reddiſh yellow, ſlightly inclining to the colour of nickel, or to that of artificial pyrites. The texture of this ſubſtance is granulated, and not very ſtrongly connected: when powdered, it is of a black colour. This pyrites is not attractable by the magnet; and is irregularly diſtributed through the ſubſtance of the ſtone.

Metallic malleable iron.

The third of theſe ſubſtances conſiſts in ſmall particles of iron, in a perfectly metallic ſtate, ſo that they may eaſily be flattened or extended, by means of a hammer. Theſe particles give to the whole maſs of the ſtone, the property of being attractable by the magnet; they are, however, in leſs proportion than thoſe of pyrites juſt mentioned. When a piece of the ſtone was powdered, and the particles of iron ſeparated from it, as accurately as poſſible, by means of a magnet, they appeared to compoſe about $\frac{2}{100}$ of the whole weight of the ſtone.

Whitiſh gray, crumbly earth.

The three ſubſtances juſt deſcribed, are united together by means of a fourth, which is nearly of an earthy conſiſtence. For this reaſon, it is eaſy to ſeparate, with the point of a knife, or even with the nail, the little globular bodies above mentioned, or any other of the conſtituent parts of the ſtone you may wiſh to obtain. Indeed the ſtone itſelf may readily be broken, merely by the action of the fingers. The colour of this fourth ſubſtance, which ſerves as a kind of cement to unite the others, is a whitiſh gray.

The cruſt is probably black oxide of iron.

The black cruſt with which the ſurface of the ſtone is coated, although it is of no great thickneſs, emits bright ſparks, when ſtruck with ſteel: it may be broken by a ſtroke with a hammer; and ſeems to poſſeſs the ſame properties as the very attractable black oxide of iron. This cruſt is, however, like the ſubſtance of the ſtone, here and there mixed with ſmall particles of iron

in

in the metallic ſtate: they may eaſily be made viſible, by paſſing a file over the cruſt, as they then become evident, on account of their metallic luſtre. This is more particularly the caſe with reſpect to the cruſt of thoſe ſtones which remain to be mentioned, they being much more rich in iron than that I have juſt deſcribed; a circumſtance I think it needleſs to repeat, in the following deſcriptions of them. The ſtone now treated of, does not, when breathed upon, emit an argillaceous ſmell: the ſame remark may be applied to all the others.

The ſpecific gravity of this ſtone is 3352.

STONE FROM YORKSHIRE.

This ſtone, the conſtituent parts of which are exactly the ſame as thoſe of the ſtones from Benares, differs from them, however, Stone from Yorkſhire. Compoſed like the former; but of finer grain:

Firſt. In having a finer grain.

Secondly. That the ſubſtance deſcribed as being in the form of ſmall globular or elliptical bodies, is not ſo conſtantly in thoſe forms, but is alſo found in particles of an irregular ſhape; a circumſtance that is not met with in the other ſtones: theſe bodies are likewiſe, in general, of a ſmaller ſize. The globules are leſs regular:

Thirdly. The proportion of martial pyrites, which has preciſely the ſame characters as that in the ſtones from Benares, is leſs; on the contrary, that of the iron in a metallic ſtate, is much greater. The quantity I was able to ſeparate by means of the magnet, appeared to me to compoſe about eight or nine parts, in one hundred, of the weight of the whole maſs. I obſerved many pieces of this iron, of a pretty conſiderable ſize; one of them, taken from a portion of the ſtone I had powdered, in order to ſeparate the iron, weighed ſeveral grains. The pyrites in leſs quantity; and the metallic iron more.

The part of the ſtone which is in an earthy ſtate, and which ſerves to connect the other parts together, has rather more conſiſtence than that of the preceding ſtones; and its appearance does not differ much from that of decompoſed felſpar or kaolin. The ſtone itſelf, therefore, although by no means hard, is rather more difficult to break with the fingers. Its earthy part is more conſiſtent.

The ſpecific gravity of this ſtone is 3508. It is denſer.

STONE FROM ITALY.

This ſtone was in a perfectly entire ſtate; conſequently, its whole ſurface was covered over with a black cruſt peculiar to all Stone from Italy.

all stones of this kind. As the stone was of a very small size, it became necessary to sacrifice the whole of it to the investigation of its nature. Its grain was coarse, similar to that of the stones from Benares: in it might be perceived the same gray globular bodies, the same kind of martial pyrites, and the same particles of iron in the metallic state. The proportion of these last was much less than in the stone from Yorkshire; but rather greater than in the stones from Benares. The same kind of gray earthy substance served to connect the different parts together; and nothing more could be perceived, except a few globules, which consisted wholly of black oxide of iron, attractable by the magnet, and one single globule of another substance, which appeared to differ from all those we have already described. This last substance had a perfectly vitreous lustre, and was completely transparent: it was of a pale yellow colour, slightly inclining to green; and its hardness was rather inferior to that of calcareous spar. The quantity of it, however, was too small to be submitted to such an investigation as might have determined its nature. The black crust which covered the stone, was rather thinner than that of the stones already described; and seemed to have undergone a kind of contraction, which had produced in it a number of fissures or furrows, thereby tracing upon the surface the appearance of compartments, similar in some measure to what is observed in the stones called Septaria.

coarse grained; composed nearly like the others.

It contained a yellow transparent globule, softer than calcareous spar.

The specific gravity of this stone was 3418.

STONE FROM BOHEMIA.

Stone from Bohemia. Composed like the preceding;

The internal structure of this stone is very similar to that of the stone from Yorkshire. Its grain is finer than that of the stones from Benares: in it may be observed the same gray substance, both in small globules and in particles of an irregular shape; also the same particles of metallic iron. The same kind of earthy substance likewise served to connect the other parts together.

This stone, however, differs materially from the others.

but its pyrites very minute; the quantity of metallic iron much greater;

First. The particles of pyrites cannot be seen without a lens.

Secondly. It contains a much larger quantity of iron in the metallic state; insomuch, that the proportion of that metal, separated from it by means of the magnet, amounted to about $\frac{25}{100}$ of the weight of the whole.

This

This ftone has alfo (owing perhaps to its having remained a much longer time in the earth than the preceding ones, all of which were taken up nearly at the very inftant of their fall) another difference, viz. many of the particles of iron in a metallic ftate, have undergone an oxidizement at their furface; a circumftance that has produced a great number of fpots, of a yellowifh brown colour, and very near to each other, over a part of its internal fubftance. This oxidizement, by adding to the bulk, and to the force of action, of the part we have defcribed as ferving by way of cement to the other conftituent parts of the ftone, has occafioned a greater degree of adhefion between thefe parts, and has rendered the fubftance of the ftone more compact. and more oxided

The great quantity of iron in a metallic ftate which this ftone contains, added to its greater compactnefs, makes it capable of receiving a flight degree of polifh; whereas it is impoffible to give any polifh to the others. When polifhed, the iron becomes very evident, in the polifhed part; appearing in the form of fmall fpecks, almoft clofe to each other, which have the colour and luftre peculiar to that metal: thefe fpecks are, in general, nearly of an equal fize. It is harder, and admits a flight polifh.

The black cruft of this ftone is fimilar to that of the others.

The fpecific gravity of the ftone is 4281.

It is eafy to perceive, from the foregoing defcription, that thefe ftones, although they have not the fmalleft analogy with any of the mineral fubftances already known, either of a volcanic or any other nature, have a very peculiar and ftriking analogy with each other. This circumftance renders them truly worthy to engage the attention of philofophers; and naturally excites a defire of knowing to what caufes they owe their exiftence. Thefe ftones are not in the fmalleft degree analogous with any other known minerals.

(To be continued.)

X. An

X.

An Answer to Mr. Gough's Essay on the Theory of Compound Sounds. By Thomas Young, *M.D. F.R.S.*

To Mr. NICHOLSON.

SIR,

Introductory remarks.

I HAVE already made public, through the medium of your useful Journal, one vindication of my remarks upon Dr. Smith's Harmonics; and I shall now beg you to insert some further observations upon a subject nearly similar. I do not think myself bound to reply to all the arguments that may happen to be advanced against any of my opinions; but when they come from persons of literary respectability, and especially when they convey an imputation of having detracted from the merits of others, I cannot forbear attempting to answer them; and in the present instance a very few words will be sufficient for the purpose. Mr. Gough has published in the last volume of the Manchester Memoirs, a paper in which he professes to defend Dr. Smith's opinions against mine: but he has unfortunately so far mistaken the question between us, that with respect to the principal circumstances, while he imagines he is confuting me, he is completely on my side.

The author's inferences from coalescence of undulations as to the doctrines of sound and light, scarcely to be called a new theory.

Proceeding upon the grounds of the well known facts relative to the grave harmonics, or the third sounds produced by the coalescence of two others, as well as upon the allowed principles of the composition of motion, I had drawn a number of inferences, which appeared to be of some importance in the first place with respect to the doctrine of sound, and which have since furnished me with still more interesting conclusions with regard to light. I was not aware that there was any probability of the justice of my inferences being disputed, or that there was so much novelty in the mode of obtaining them, as to deserve the name of a theory or an invention: but if it be deemed otherwise, I shall always be ready to acknowledge the invention with satisfaction, and to support the theory with alacrity.

Dr. Smith imagined that, when undulations in

I shall now quote the words of Dr. Smith, which gave rise to my animadversions; and this statement is the more necessary,

ſary, as I before omitted to detail the particulars of that author's miſconception, and as Mr. Gough appears wholly to have overlooked the paſſage. "Different particles of the air at the ear" ſays Dr. Smitth, "will keep moving conſtantly oppoſite ways at the ſame time. And in ſo rare a fluid as air is, where the intervals of the particles are eight or nine times greater than their diameters, there ſeems to be room enough for ſuch oppoſite motions without impediment: eſpecially as we ſee the like motions are really performed in water, which in an equal ſpace contains eight or nine hundred times as many ſuch particles as air does. For when it rains upon ſtagnating water, the circular waves propagated from different centres, appear to interſect, and paſs through, or over each other, even in oppoſite directions, without any viſible alteration in their circular figure, and therefore without any ſenſible alteration of their motions." Harmonics, 1759, p. 105.

air or in water croſs each other, different particles of the fluid were at the ſame time moved *inter ſe*, according to the reſpective undulations.

It certainly would have "coſt me an effort of ſtudy" to demonſtrate this, although I could not exactly conſider it as the "intuitive concluſion of a comprehenſive mind." Such a mind appeared to me to comprehend with equal eaſe the diſtinct and the confuſed; and I am only at a loſs to conceive how the writer of this paſſage could ever have compoſed a valuable treatiſe on optics. Mr. Gough's theory differs as widely from this ſtatement as mine does. My remark on it was this; "It is ſurpriſing that ſo great a mathematician as Dr. Smith could for a moment have entertained an idea, that the vibrations conſtituting different ſounds, ſhould be able to croſs each other in all directions, without affecting the ſame individual particles of air by their joint forces: undoubtedly they croſs, without diſturbing each other's progreſs; but this can be no otherwiſe effected than by each particle's partaking of both motions. If this aſſertion ſtood in need of any proof, it might be amply furniſhed by the phenomena of beats, and of the grave harmonics obſerved by Romieu and Tartini, which Mr. Lagrange has already conſidered in the ſame point of view." Phil. Tranſ. 1800, p. 130.

Whereas the ſame individual particles of the fluid receive and tranſmit both undulations.

I have no objection to admitting the whole of Mr. Gough's propoſitions, in the particular caſes which he has conſidered; but when he ſays that the coaleſcence of two ſounds is impoſſible on mechanical principles, he thinks proper to omit the only caſe in which I had aſſerted its exiſtence, that is, when

Sounds may coaleſce which arrive at the ear in the ſame direction;

 the

the founds arrive at the ear "in the fame direction." p. 131. Surely Mr. Gough will not deny the poffibility of fuch a coincidence in direction, fo far at leaft that the phyfical effect may be the fame as if the coincidence were perfect: when, for inftance, we liften to two or more founds paffing through a long tube, or to the various fubordinate founds of the fame chord or pipe. If he be actually difpofed to deny the perfect coincidence in practical cafes, I fhall only appeal to experience, which fhows that the effect of a third found is moft diftinctly heard, whenever the theory leads us to expect it; but there will always be a much greater portion of each found fo reflected from the furrounding objects as not to coincide in direction fufficiently for coalefcence, and hence the original notes will always be much more audible than the new compound. The ear indeed appears to have greater powers of analyfis than one would naturally have expected, it decompofes a "compound" juft as if it were a mere "mixture," not only in this cafe but in many others: how it performs this operation, I do not pretend to determine. When Mr. Gough reprefents me as maintaining that two mufical ftrings, differing in the times of their vibration, and happening to vibrate in concert, do not occafion two diftinct founds, and that the waves of air are compelled by their mutual interference to coalefce, thereby producing a new fucceffion of pulfes, conftituting a fingle found in the place of the former; his expreffions tend to impute to me an opinion which could only be maintained by a perfon who had never heard a fingle mufical compofition, or ever been prefent at the converfation of a mixed company.

not by lofing their refpective tones in one fingle found; but by producing an additional weaker found.

All imperfect unifons muft produce beats.

I am not folicitous for the application of the term compound by coalefcence, to the human voice; but Mr. Gough can fcarcely form to himfelf a diftinct conception of it, very different from mine. A mixture of imperfect unifons would inevitably be accompanied by the production of beats; and if he affert that the imperfection is too fmall to produce this effect, I will only requeft him to affign any reafonable limit to its magnitude, and by producing the note long enough, I will fhow that a beat muft neceffarily enfue. Perhaps a wifh to retain the Newtonian theory of the law of the undulations may have led him into thefe fuperfluous refinements.

If

If Mr. Gough will take the pains of examining the phenomena of the grave harmonics, which he feems to have hitherto thought beneath his notice, he will be convinced that the coalefcence of mufical founds is not only poffible, but of very frequent occurrence, and that the compound found does actually "acquire fenfible properties peculiar to itfelf;" and if he will explain, in any other manner than I have done, the indubitable fact of the audible impreffion of the prefence of the fourth below the key, in confequence of the coexiftence of the founds conftituting the interval of a major third, even when both the notes are freed from their harmonics, and when all echo is avoided; I fhall then liften to him, with the attention due to a perfon who endeavours to regulate his arguments by the refults of accurate obfervation.

The phenomena of the grave harmonics fhew that mufical founds coalefce in numerous inftances.

I am, Sir,

with great refpect,

Your obedient humble Servant,

THOMAS YOUNG.

Royal Inftitution, May 30, 1802.

CORRECTION.

In your fifth volume (quarto) p. 166, l. 20, *for* " a better third than that," *read*, " equal to the third."

The following CORRECTIONS *made by Dr.* YOUNG *in his Paper on the Mechanifm of the Eye, which is inferted in our fifth volume (Quarto Series) were omitted to be made in their proper places.*

Page 256, line 6, Prop. III. *after e, infert* the bafe being unity.

Page 257, line 15, Cor. 10. *for* ntu, *read* ntt; line 16, *for* product &c. *read* fquare of the cofine of incidence.

Page 258, line 2, Cor. 11. *for* $1 + u^2 - 2u^4$, *read* $2muu$.

Page 258, Prop. V. Cor. See the note in p. 299.

Page 259, Prop. VIII. By a miftake of a fign, the eighth propofition is rendered erroneous; no ufe having been made of that propofition, it has been inferted without proper revifion. It ought to ftand thus, with its demonftration:

PROPOSITION VIII. PROBLEM.

To find the path of a ray of light falling obliquely on a fphere, of a refractive denfity varying as any power of the diftance from the centre.

The

The refractive density, in the sense of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium.

(Schol. 2. Prop. I.) Let the velocity at the distance x be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we have, in Prop. 41. l. 1. Princip. $\sqrt{ABFD} = x^{-\frac{1}{r}}$, $Q = s$, the sine of incidence, the radius being unity, $Z = sx^{-1}$, $De = \frac{s}{2xx\sqrt{x^{-\frac{2}{r}} - s^2 x^{-2}}} = \frac{1}{2} s x^{\frac{1}{r}-2} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\,\Big|^{-\frac{1}{2}}$, and the fluxion of the area described by the radius $= -\frac{1}{2} s x^{\frac{1}{r}-2}$ $\dot{x} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\,\Big|^{-\frac{1}{2}}$. Let the sine of the inclination to the radius at each point be called y; then $y = s x^{\frac{1}{r}-1}$, $\dot{y} = \frac{1-r}{r} s x^{\frac{1}{r}-2}$ $\dot{x}$, and the fluxion of the area $= \frac{r}{2r-2} \dot{y} \cdot \overline{1 - yy}\,\Big|^{-\frac{1}{2}}$, of which the fluent is $\frac{r}{2r-2}$ Y, y being the sine of the arc Y; and the angle corresponding is $\frac{r}{r-1}$ Y. The value of that angle being found for any two values of x or y, the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as r to $r-1$, and the deviations is to that difference as 1 to $r-1$.

Corollary. Hence, in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

Page 259, line 9, *for* but the two last, &c. *read* the seventh may either be deduced from the eighth, or may be demonstrated independently of it.

Page 264, last line, *after* internally, *insert* Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17.

XI. *Experiments*

XI.

Experiments on the Velocity of Air iſſuing out of a Veſſel in different Circumſtances; with the Deſcription of an Inſtrument to meaſure the Force of the Blaſt in Bellows, &c. By Mr. BANKS, *Lecturer in Natural Philoſophy* *.

THE object of this inquiry may be announced in the following propoſition: If an elaſtic fluid is generated in a given veſſel, or any way contained in it, and at liberty to iſſue out of the ſaid veſſel through a given aperture, to determine the reſiſtance which the veſſel meets with from its action, or the power which it has of communicating motion to the veſſel, as in a ſky-rocket, Sadler's ſteam engine, &c.

Propoſition. To determine the mechanic force or reaction of an elaſtic ſpouting fluid.

Before we proceed to relate the experiments, it may be proper to premiſe certain principles deduced from theory. If a tube be filled with any kind of fluid, as *air, water, mercury,* &c. and placed in a vacuum, every fluid will flow out with the ſame velocity. For though the preſſure of a column of mercury of a given altitude be much greater than an equal column of water, yet the weight of the particles to be projected is greater in the ſame ratio. On the other hand, if air is lighter than water, the particles projected are alſo lighter in proportion. If a tube of 16 feet high be filled with air of any denſity, that air, like water, would flow into a vacuum with a velocity of 32 feet per ſecond, no corrections being made for reſiſtance †.

All uniform fluids flow from an orifice with the ſame velocity *in vacuo*, if their heights be equal, however different their denſities.

And if we take the gravity of air to water as 1 to 840, then a column of one foot of water compreſſing air, will produce as great a velocity in that air as a column of air 840 feet high, ſuppoſing it was of uniform denſity.

If we take the whole preſſure of the atmoſphere equal to 33 feet of water, or its height (ſuppoſing it to be equally denſe,

Equal velocities will be produced in one iſſuing fluid by the preſſure of another; if the height of the former to that of the latter, be inverſely as its denſity.

* From the Mancheſter Memoirs, Vol. V. p. 398.

† In the ſuppoſition of a perpendicular tube open at the top, filled with air or any elaſtic fluid, the author takes the denſity of the column at the bottom, or where the aperture is made, to ariſe ſolely from the weight of the elaſtic column; and the altitude to be that which would be if the whole column were reduced to the denſity of that at the bottom.

which

The atmosphere presses equal to 33 feet of water, or 27720 feet of air. It would therefore propel air into a vacuum with the velocity due to this fall: *viz.* $\sqrt{16} : \sqrt{27720} :: 32$ feet : 1332 feet per second.

which in this case will make no difference) equal to 33 multiplied by 840, or 27720 feet; then as the square root of 16 is to 32, the velocity at the depth, so is the square root of 27720 to 1332 feet per second, the initial velocity of the atmosphere into a vacuum.

Experiments. Air is compressed in a vessel by a known height of water:

To prove whether air compressed by 33 feet of water would be impelled into the atmosphere with the above velocity, I have made, amongst many more, the following experiments:

A, plate xiv. fig. 1 *, is a vessel of a known capacity, into the top of which is screwed an aperture of a known area. The tube *Td*, recurve at *d*, is soldered or screwed into the top of the said vessel. The hole *a* is stopped, and water poured into the tube at *T* till it is full, at which time a quantity of water will have passed out of the tube at *d*, and condensed the air in the vessel, more or less as the tube *T d* is longer or shorter.

and suffered to issue through an aperture of known dimensions. Its velocity is known from the quantity and the time.

At this time a person who has closed the aperture at *a* with a finger of one hand, and held a half second pendulum in the other, removes both at the same time, while at the same moment an assistant opens a cock over the tube *T*, which supplies it with water as fast as it can descend into *A*. The moment that the water appears at *a*, the time-piece is stopped, and the time of expelling the air is noted, from which, by knowing the capacity of the vessel, the velocity may be obtained.

If the tube *Td* should be continued near the bottom of the vessel *A* while it was filling with water, the length of the compressing column would be gradually diminishing, and of consequence the pressure would be constantly changing; hence the open end of the tube is as near the top of the vessel as is consistent with a free passage for the water.

EXPERIMENTS.

The vessel *A* contained 15 lb. 6 oz. of water, from which we find its capacity is 425.088 cubic inches.

The area of the aperture *a*, through which the air is expelled, .0046 inches.

* There being no figure in the Transactions, I have inserted the drawing referred to. N.

Exper.

Exper. I. The altitude of *T* above the veſſel is 30 inches. Time of expelling the air, by ſeveral trials, is 33 ſeconds. Experiment I. with a fall of 35 inches.

Exper. II. The altitude of *T* is ſix feet. The time of filling, by ſeveral trials, is 21.3 ſeconds. Experiment II. with a fall of 72 inches.

In the firſt experiment, 425.088, the ſolidity of the veſſel, divided by .0046, the area of the hole *a*, gives 92410.4 inches for the length of the ſtream of air driven out in 33 ſeconds; divide that length by 33, and we ſhall have 233.3 feet, the velocity per ſecond, communicated by 30 inches of water. Computation of the firſt experiment gives actual velocity per ſec. = 233.3 feet;

The ſecond experiment by the ſame proceſs gives 361.6 feet per ſecond. If we would compare theſe together, we may ſay, as the ſquare root of 30, the head, is to 233.3, the velocity; ſo is the ſquare root of 72, the ſecond head, to 361.8 feet, the velocity per ſecond. and of the ſecond gives 361.6 per ſecond.

Again:—As the ſquare root of 6 feet is to 361.6, ſo is the ſquare root of 33 feet to 845.2 feet per ſecond, the velocity produced by that head, or the initial velocity with which the atmoſphere would enter a vacuum. This velocity, found by experiment, is 487 feet per ſecond leſs than has been aſſigned by theory. Whence the velocity of air from the atmoſphere into a vacuum is computed, and turns out to be 845.2 inſtead of 1332 feet, as by theory.

It appears however that the reſults, as determined by theory and experiment, do not differ more than in the caſe of effluent water. For, if we would reduce the velocity of effluent water found by theory, to that which experience gives, we muſt multiply it by .634. Accordingly, if we multiply 1332 feet, the velocity of the atmoſphere entering a vacuum as calculated above, by .634, the product is 844.5 per ſecond, differing but $\frac{7}{10}$ of a foot from that juſt found by experiment. But this reſult, corrected by the multiplier given by experience for effluent water, proves to be very correct.

I have alſo made experiments by ſinking veſſels in water, till their tops were even with its ſurface, and opening the aperture that the riſing water might expel the air, by which I obtained the ſame velocities as above; but the method of computing is much more intricate, for which reaſon I ſhall not inſert them. Experiments with riſing water.

From the above it appears, that a preſſure equal to 33 feet of water, will expel air out of the bellows into the atmoſphere with a velocity of 845 feet per ſecond; that *one foot* of water in depth will produce a velocity of 147$\frac{1}{4}$ feet, and *one inch* a velocity of 42 feet per ſecond, or 20 miles an hour. General reſult:

Hence we may conſtruct a table ſhewing the velocity communicated to air by any head of water; for as the ſquare root of applied to the conſtruction of tables for effluent air preſſed by water;

of 6 feet is to the velocity produced by that head, so is the square root of any other depth to the velocity produced by that depth.

or in bellows,

We may also, from the above, construct an instrument which will shew the velocity with which air flows out of any kind of bellows, with as much accuracy, as the experiments have been made on which its construction depends.

Description of the Instrument, &c.

by means of an instrument here described. It is a portable vessel containing water, to be formed into an open gage tube;

The metal box or tube B, plate xiv. fig. 2, may be about the size of the figure; the top must be made air tight by the cover L; into the bottom is fixed the small tube $A\,C$, and into the piece D is cemented the glass tube $E\,D$: the instrument is then inverted, and some water poured through the tube $A\,C$, till, when in its proper position, it is visible at D. It is now ready for use, and the end A may be fixed in a hole made in the upper board of the bellows, and the water will rise in the glass tube, in smith's bellows, generally from 9 to 12 inches, furnace bellows, generally four feet or more. But where the compression is great, quicksilver may be used instead of water, only in this case the instrument should be made of iron, as quicksilver causes the screws of brass to break. Or, instead of quicksilver, the tube $E\,D$ may be sealed at the top E, and then a length of 12 or 18 inches will be enough for any blast. The glass tube needs not be more than one-eighth or one-tenth of an inch in diameter.

Whatever compression there may be in the bellows, there will be the same in the upper part of the tube B, which will force the water into the glass tube $D\,E$, and make the air in its upper part of the same density, deducting from the compressing force the altitude of the water raised above D, which however will be of little or no importance; if the gauge is placed in a horizontal position, with the glass tube downward, there will be no difference of density.

or the tube may be hermetically closed.

The computation for the force in the case where a tube hermetically sealed at the top is adopted in the instrument, will be effected by considering that the space occupied by any elastic fluid is inversely as its force. Thus, let the tube be 12 inches long, and suppose the water to be raised one inch, then it will be 11 : 12 :: the force of the atmosphere : the force of the air in the tube :: 1 : $1\frac{1}{11}$. Hence a scale may be adapted to the instrument,

inftrument, to exprefs the force of condenfation over and above the common atmofpheric preffure; which force is fignified in the inftance above, by the fraction $\frac{1}{11}$, unity being the atmofpheric preffure. If we denote the atmofpheric preffure by 30 inches of mercury, or 32 feet of water, then the force $\frac{1}{11}$ in the above example, will be expreffed by 2.727 inches of mercury, or 2.91 feet of water; and the like for any other inftance.

If mercury be ufed, its denfity muft be confidered in the fhort inftrument.

If a mercurial inftrument of the above conftruction be preferred, it becomes neceffary to add the height of the mercurial column to the force found as above: thus, if the condenfation of air be from 12 into 9 inches, then the addition to the force of the internal air in the tube is equal to $\frac{1}{3}$, or 10 inches of mercury, to which muft be added the three inches raifed in the tube, and the whole force will be 13 inches of mercury, exclufive of that of the atmofphere.

General application to all gafes.

This fort of inftrument or gauge ferves equally well for finding the expanfive force of any kind of elaftic fluid, as for meafuring the velocities with which they iffue out of the place of their confinement. It may be applied to all kinds of bellows, to condenfed fteam, and to the air pump.

XII.

On the Variation of Rate in a Time-Piece, as indicated by the Changes in the Arc of Vibration. In a Letter from Mr. Ezekiel Walker.

To Mr. NICHOLSON.

SIR, *Lynn, July* 20, 1802.

Expediency of frequent obfervation of the arc of vibration in clocks.

FROM what I have already mentioned in a former paper *, it follows, that in clocks ufed in making aftronomical obfervations, it is neceffary to obferve the arc of vibration very frequently, and when it is found different from that generally defcribed by the pendulum, the rate of the clock muft on this account be corrected.

Rule for deducing the variation in time from that of the arc.

To determine this correction, let x denote the time which a pendulum, vibrating in an arc exceedingly fmall, will lofe when made to vibrate in a larger arc of the fame circle, T the number

* See pa. 76 of this vol.

of seconds in 24 hours (86400), and D the number of described on each side of the perpendicular *.

$x = T \times \frac{D^2}{52480}$ nearly. Consequently $x = D^2 \times$ nearly.

the above theorem the following table was computed, shews the time lost by a pendulum in 24 hours, by in- its semi-arc of vibration in the same circle 1′ of a

Table of ... tions ... pond ... their ...

Half the arc of ... of a ... in a	Loses per day of a pendulum vibrating the least	Difference.	Loses per day on increasing its semi-arc of vibration 1′ of a degree.
	0,″0		
		0,″1	,006
0 ¼	0, 1		
		0, 3	,020
0 ½	0, 4		
		0, 5	,033
0 ¾	0, 9		
		0, 7	,046
1	1, 6		
		0, 9	,060
1 ¼	2, 5		
		1, 2	,080
1 ½	3, 7		
		1, 3	,086
1 ¾	5, 0		
		1, 6	,107
2	6, 6		
		1, 7	,113
2 ¼	8, 3		
		2, 0	,133
2 ½	10, 3		
		2, 1	,140
2 ¾	12, 4		
		2, 4	,160
3	14, 8		
		2, 6	,173
3 ¼	17, 4		
		2, 8	,187
3 ½	20, 2		
		3, 0	,200
3 ¾	23, 2		
		3, 2	,213
4	26, 3		

* Simpson's Fluxions, Art. 464.

The

The ufe of this table will be eafily underftood by the following examples.

EXAMPLE I.

Suppofe a clock goes mean folar time when the pendulum vibrates 2° on each fide of the perpendicular, what will it lofe per day when the pendulum vibrates through an arc of three degrees? Examples

Againft 2° and 3° in the firft column, we have 6,″6 and 14,″8 refpectively in the fecond column. Then 14,″8 — 6,″6 = 8,″2 = the time loft per day.

EXAMPLE II.

Suppofe a clock gains 1″ per day, when the pendulum vibrates 1° 49′ on each fide of the perpendicular, what would be its rate when the pendulum vibrates 1° 51′?

Between 1°¾ and 2° in the firft column, we have ,″107 in the fourth column. Then $1'' - ,''\overline{107 \times 2} = 0,''786$ = the daily rate of the clock gaining.

I am, with refpect,

Sir,

Your humble fervant,

E. WALKER.

ANNOTATION.

Whether theory can be applied to an engine fo compounded as a clock, &c.

The arc of vibration in clocks is likely to be affected in practice by changes in the denfity of the air, in its temperature, and in the action of the firft mover; and thefe caufes will moft probably afford refults confiderably different from thofe inferred from the fimple pendulum in vacuo. I take it to be experimentally afcertained by the regifters of the performance of aftronomical clocks very firmly fixed, that the gridiron pendulum, with a very flexible fpring fufpenfion, is not fenfibly affected during the different ftations of the barometer and thermometer; and I apprehend that the firft mover, if a weight, muft operate either with a conftant force or with periodical variations, occafioned by the train or mechanifm through which its action is tranfmitted, the influence of oil, &c. which periods are rendered fhorter by the well known methods of difengaging the efcapement, or parts neareft the pendulum. Many other confiderations will offer themfelves to the learned author, as to

thoſe parts of the vibration to which the maintaining power may be applied ſo as to increaſe the arc, while the time may be either lengthened or ſhortened according to the circumſtances, the general nature of the eſcapement, &c. and theſe will probably lead to a concluſion, that the corrections of time to be applied to a clock, which either in its arc of vibration or otherwiſe gives ſymptoms of irregularity, can be gained only from direct obſervation.

W. N.

XIII.

Deſcription of Atkins's Hydrometer for aſcertaining the ſpecific Gravities of ſpirituous Liquors. By J. FLETCHER, *Eſq. Communicated by the Author.*

Utility of deſcriptions of philoſophical inſtruments.

AMONGST the various papers which are to be found in the works of thoſe journaliſts who have ſo materially contributed to the advancement of our knowledge, there are perhaps but few which have been more effectually conducive to this end than thoſe which are appropriated to the deſcription of the inſtruments of ſcience. It is indeed much to be lamented, that the merchant, the manufacturer, and the artizan, ſhould continue to be ſo generally deprived of the advantages to be obtained from the uſe of ſuch of them as are adapted to their purpoſes, for want of the neceſſary information with regard to the mode of applying them.

The inſtrument which is here intended to be deſcribed, is one of thoſe which ſtand in a great meaſure in this predicament; and as it appears very well to deſerve a deſcription, it is conceived that, imperfect as the following one is, it will not be unacceptable.

Spec. gr. only criterion of the ſtrength of ſpirits.

It is now univerſally acknowledged, that the ſpecific gravities of ſpirituous liquors afford the only tolerable criterion of their ſtrength, and conſequently of their comparative values. To aſcertain the ſpecific gravity of a liquid with a conſiderable degree of preciſion, is an operation of no great difficulty. The moſt accurate method of performing it is perhaps that which is alſo the ſimpleſt: to weigh the fluid in a veſſel in which its bulk is capable of being nicely meaſured. Much more, however,

ever, remains to be done in order to the eſtimation of the ſtrengths of ſpirtuous liquors: the contraction of the maſs of a compound of alcohol and water on their mixture, and the variation in its meaſure in reſpect of temperature, are each of them of ſufficient practical importance to render their appreciation neceſſary when the value of the ſpirit is to be diſcovered. Thus for example, 18 gallons each of alcohol and water will produce only 35 gallons of the compound, and a difference of 30° in temperature of Fahrenheit's ſcale, occaſions ſuch a change in the ſpecific gravity of proof ſpirit, as, if omitted to be taken into conſideration, would render the dealer liable to an error of upwards of ten per cent. in the eſtimation of its ſtrength and conſequent value.

Corrections neceſſary to be applied for concentration and temperature.

18 parts each of alc. and water produce only 35 of the mixture.

A variation of 30° of temp. occaſions a change of 10 per cent. in the apparent ſtrength of proof-ſpirit.

In commercial tranſactions with reſpect to ſpirituous liquors, it is neceſſary to appreciate their ſtrength by comparing it with that of ſpirit of a certain ſuppoſed quality in this reſpect as a ſtandard. This ſtandard-ſpirit (which is called *proof-ſpirit*) is of ſuch a degree of ſtrength that its ſpecific gravity is about .920 at 60° of Fahrenheit's thermometer; and the object of enquiry in all caſes is with regard to the quantity of this proof ſpirit which would be equivalent to a given quantity of any ſpirit under examination. The language of the ſpirit-dealers with reſpect to their terms of "*over-proof*" and "*under-proof*," has in all caſes this kind of reference to commercial value. When they ſay that a certain kind of ſpirit is 30 per cent. over proof, they mean that if 100 parts by meaſure be increaſed to 130 by the addition of water, it will become of proof ſtrength; and when they ſay that it is 30 per cent. under proof, they mean that 70 parts by meaſure of proof-ſpirit will become, when increaſed to 100 by the addition of water, of equal ſtrength to that of the ſpirit in queſtion. If therefore a gallon of proof ſpirit be worth 8s. 4d. the ſame quantity of the former kind of ſpirit will be worth 10s. 10d. and of the latter 5s. 10d.

Commercial eſtimation of value of ſpirituous compounds, by compariſon with proof-ſpirit.

Definition of terms "over-proof" and "under-proof."

It is not therefore ſufficient for the purchaſer of a ſample of ſpirit, or the officer who is to collect the duties on it, to be informed of its ſpecific gravity at a given temperature: it is neceſſary for him to aſcertain the quantity of proof-ſpirit which is capable of producing, or being produced, by the ſpirit in queſtion by the addition of water only, and which is conſequently equivalent to it in value.

The deſideratum is to find the equivalent quantity of proof-ſpirit.

The

Calculation inapplicable to this purpose.

The solution of this problem being almost impossible by any application of mathematical formulæ, it is of course desirable to be enabled to obtain it instrumentally, or by inspection of tables previously laid down from experiment; and the use of the latter, under such circumstances as those in which operations of this kind are generally performed, having been found inconvenient, its instrumental solution has been universally preferred. This has been attempted principally in two ways: by the hydrometer, with a multiplicity of weights adapted to the various corrections for temperature; and by the same instrument more simply constructed, so as to indicate merely the specific gravity of the liquor; the necessary corrections being applied by a scale or sliding-rule.

Inconvenience of tables.

Instrumental solution preferable.

The hydrometer with many weights,

with a sliding-rule.

The latter most eligible.

The instrument here to be spoken of is constructed on this latter principle, which appears for many reasons to be the best: The hydrometer A B (pl. XV. fig. 3) is of brass. It is eight inches in length, with an elliptical bulb an inch and a half in diameter, and two inches long. The stem is square, each side being about $\frac{1}{8}$ of an inch wide, and, when the instrument is intended for such liquors only as are specifically lighter than water, is engraved only on one face with the 26 letters of the alphabet, and an O, or zero, at the top and bottom; opposite to each, and between every two of which, is a division for marking the point of the stem which is cut by the surface of any liquor in which it floats, the whole number of divisions being 55, as shewn in the plate. The weight of the instrument is about 400 grains, and it is provided with four weights, marked 1, 2, 3, 4, weighing respectively 20, 40, 61, and 84 grains, to be applied as occasion requires on the shank of the instrument C, on which they are retained by the button or fixed weight B. These weights are so adjusted, that when with any one of them, as for instance No. 2, the stem of the instrument, when floating in a given liquor, just emerges to the lower division O, it will, on changing the weight for the next heavier, No. 3, become immersed exactly to the other division O, towards its superior extremity. The stem is by this means virtually extended to five times its real length, and the number of divisions in effect augmented to 272. Thus without any weight at all, as represented in the figure, it would sink in a liquor whose specific gravity was .806, exactly to the upper division O,

Description of Atkins's hydrometer.

Square stem, with the letters of the alphabet on one face,

and four weights.

When it emerges to the bulb with one weight, the next heavier sinks it to the top of the stem.

Stem and divisions thus in effect multiplied by 5.

With the weights singly applied, indicates specific gravities from .806 to 1.000;

O, and in one whofe fpecific gravity was .843, to the lower divifion O; the intermediate divifions anfwering to intermediate fpecific quantities. With No. 1, it indicates fpecific gravities from .843 to .880; with No. 2, from .880 to .918; with No. 3, from .918 to .958; and with No. 4, from .958 to 1.000; the furface of water in which the inftrument is made to float with the latter weight, exactly coinciding with the lower O, at 55° of temperature.

The whole interval from .806 to 1.000, comprehending the fpecific gravities of all fpirituous compounds, is in this manner divided into five nearly equal portions, each of which, comprizing from .038 to .040, is meafured by the whole length of the ftem; fo that each of the 54 intervals on its face correfponds to confiderably lefs than an unit in the third place of the fpecific gravity, and indicates a difference of about one half per cent. or two quarts in 100 gallons, in the quality of the compound, and the inftrument is therefore abundantly fenfible to very minute variations in thefe refpects.

each length of the ftem anfwering to about .04; and each divifion to lefs than .001 in fpec. gr. or ½ per cent. in ftrength.

With regard to the application of the weights, no error can poffibly be committed. If the inftrument floats in any liquor, fo that its furface cuts any part of the ftem, it is properly loaded. Any weight but the proper one will either fink it entirely, or fuffer the ftem to rife totally above the fluid.

The fpecific gravity indicated by any divifion on the ftem of the hydrometer, is feen inftantly by reference to a fliding-rule belonging to it, whofe two faces are fhewn, plate XV. fig. 2 and 3, on which the lines of divifions A A, A A, marked with the letters of the alphabet, reprefent thofe on the inftrument when loaded with the weight, whofe number is marked over the O at the commencement of each feries, and the exterior lines of divifions B B, B B, near its edges, fhew the correfponding fpecific gravities compared with that of water at 55°.

Spec. gr. indicated by the divifions on the ftem compared with the rule.

In order, however, to afcertain the ftrength and value of the fpirit, it is neceffary to examine its temperature and apply the proper correction, which is done with extreme accuracy by the fame fliding-rule, by an ingenious application of two fcales of unequal parts to each other, *viz.* the lines C C, C C, marked "*Atkins*," and the lines A A, A A, marked with the letters of the alphabet. The mode of doing this is as follows: the temperature of the liquor being taken by the thermometer accompanying the inftrument, the afterifk or index, plate XV. fig. 1, on

Correction for temperature applied,

by fetting an index on the flider to the degree of temperature as fhewn by the thermometer.

on that part of the ſlider oppoſite to the thermometrical ſcale D, is to be ſet to the degree of temperature ſo found, and the diviſion on the line C, marked "*Atkins*," which then correſponds on either ſide of the rule with that letter and diviſion on the alphabet line A, at which the ſtem was cut by the ſurface of the liquor, indicates the ſtrength and value in commercial terms. Thus ſuppoſe the temperature to be 68° (at which the index appears ſet in the plate), and the hydrometer, when loaded with its weight No. 1, to float with the ſurface, cutting the diviſion I on the ſtem (correſponding to a ſpecific gravity of about 854½), we find, on ſetting the ſlider of the rule to 68° on the thermometer ſcale, that 46 over proof correſponds to that letter and diviſion, and is conſequently the ſtrength and value of the liquor.

Diviſion on ſlider anſwering to the letter on the ſtem gives per centage.

Example from the rule ſet as in the plate.

Uſes of the other lines on the ſlider.

The lines marked "*Dicas*" and "*Clark*" on the rule, are for giving the appreciation of the values and ſtrengths as ſhewn by the inſtruments invented by thoſe artiſts, and are intended for the uſe of ſuch as have been in the habit of employing them; the latter being perhaps inſerted principally on account of the uſe of Clark's inſtrument for the purpoſes of the revenue.

Of the concentration-line.

The figures on the line marked "*concentration*" amongſt the *over-proofs*, indicate the diminution in volume which takes place on reducing the given compound to proof; and amongſt the *under-proofs*, the diminution which takes place on reducing proof-ſpirit to that ſtrength. This muſt in the firſt caſe be deducted from the quantity marked on Atkins's line (C), in order to obtain the accurate per centage according to the eſtimation of commercial men. For inſtance, 50 over-proof on Atkins's line indicates, that if to 100 parts of ſpirit of the ſtrength which anſwers to this diviſion, we add 50 of water, we ſhall get proof-ſpirit, of which, however, the quantity will be only 147 parts; the concentration or diminution of bulk by mixture being 3 parts. With regard to the ſpirit which is under-proof, the figures on the concentration line indicate the diminution of volume in pints for every 100 gallons of the mixture: if, for inſtance, 70 gallons of proof-ſpirit be combined with 30 of water, we get a compound whoſe ſtrength is that which is marked on the line C, 30 under proof; but the concentration being ſeven pints, we ſhall get only 99⅛ gallons.

Concentration to be deducted from the per centage.

Example. Diminution of 3 parts on adding 50 of water to 100 of 50 over-proof.

Concentration on mixture of proof-ſpirit and water.

When the hydrometer is intended only for ſpirituous compounds, the weights are applied ſingly as before-mentioned: if, however, in addition to the weight No. 4, the others are ſucceſſively applied, it becomes applicable to the examination of worts and other liquors, whoſe ſpecific gravities are from 1.000 to 1.109, or, in the language of the brewery, up to 40 lbs. per barrel heavier than water. In this caſe the other three ſides of the ſtem are alſo graduated, and another rule with an ivory ſlider, carrying a thermometrical ſcale for comparing worts at different temperatures, is included in the caſe with the inſtrument.

Applicable to worts by applying the weights doubly.

Spec. gr. of worts deſignated by lbs. per barrel.

Additional rule for the brewery.

The writer of this paper having made a conſiderable number of experiments with this inſtrument, on the ſpecific gravities of a variety of ſpirituous liquors, had originally intended to have given their reſults; but it has already run to ſuch a length, that they muſt be deferred to a future communication. The errors, even including thoſe which muſt neceſſarily ariſe from the various temperatures of the compounds, and the different quantities of foreign matters with which theſe fluids, in an impure ſtate, may be ſuppoſed to be charged, appear to fall within very narrow limits; and the extreme facility and expedition with which it reſolves the queſtions to the ſolution of which it is applicable, cannot fail to render it very highly valuable to thoſe for whoſe purpoſes it is principally intended.

Errors in reſults very ſmall, and advantage of facility of uſe very conſiderable.

XIV.

An Examination of Sig. VOLTA'S *Experiments which he calls fundamental, and upon which his Theory of Galvaniſm reſts; with a Deſcription of a very ſenſible Electrical Condenſer, and an Explanation of the Action of the Electric Fluid in the Galvanic Inſtrument. By* JOHN CUTHBERTSON, *Philoſophical Inſtrument Maker, No, 54, Poland Street, London. Communicated by the Author.*

IN Vol. I. 8vo, page 136, paragraph three, of this Journal, Sig. Volta affirms, "If two inſulated diſcs, one of copper and "the other zinc, be applied together for a moment and then "ſeparated, the zinc will be poſitive and copper negative." I have always had the ſame reſult, but ſome times much weaker than at others.

Volta's expt. of the contact of inſulated diſcs of copper and zinc producing + el. in the zinc, and — el. in the copper. Succeeds.

Page

Expt. of copper and zinc joined, and the copper to touch a zinc condenfer, and afford — el. in this laft.

Page 137, in the laft paragraph, it is faid, " If a piece of " zinc to which is joined a piece of copper, and the zinc held " by two fingers or in any other manner, and the copper made " to touch the fuperior difc of the condenfer, which is zinc, " while the inferior is in communication with the ground, a " moment afterwards raifing the upper difc in the air, it will " be negatively electrified." This I have always found the contrary, *i. e. pofitive.*

Did not produce — el. but + el.

Expt. of zinc touching a copper condenfer, and affording no el.

Page 138, paragraph three, it is alfo faid, " If the fuperior " difc of the condenfer be copper, and a piece of zinc be made " to touch it immediately without any intervening fubftance, " nothing will be obtained, becaufe the zinc being then in " contact at the two oppofite ends with copper and copper, " two equal forces act in oppofite directions, and by that " means deftroy or counterbalance each other." If the fuperior difc was copper, I always found it negative, and if it was zinc touched by a piece of copper, it was pofitive; both contrary to Sig. Volta's affertion.

Did not fucceed.

We find then that only one experiment out of three fucceeds, which he calls fundamental, upon which his theory of galvanifm is founded.

The foregoing expts. were often repeated.

Knowing Sig. Volta's abilities both as a philofopher and experimentalift, I own I mention the refult of the foregoing experiments (though it is after numerous repetitions) with much diffidence; but the experiments are fo fimple, that it is almoft impoffible that I can have erred.

By reafoning upon the phenomena, when copper and zinc are made to touch each other, it becomes eafily explicable by the old known laws of electricity.

EXPLANATION.

Explanation. In expt. I. either the zinc *attracted* or the copper *repelled* the elect. while in contact.

In the firft experiment, where zinc and copper are made to touch each other, we find, on feparation, that the zinc has acquired a greater fhare of electric fluid than it had before the touch, and the copper lefs, by virtue of their mutual action upon each other when in contact. So that the zinc muft have either attracted the electric fluid out of the copper, or the copper muft have repelled it from itfelf into the zinc; and that the firft is the true caufe may be proved by connecting the copper difc with conductors, and then touching it by the infulated zinc, which will be found, on feparation, to be much more ftrongly pofitive

positive than when the copper was insulated. For whatever quantity of electric fluid may be drawn out of the copper by the zinc, is recovered by its being in connection with continued conductors; whereas, if it had been repelled from the copper into the zinc, it must be stronger when insulated, because when insulated, the copper has no other body but the zinc to receive that which it repels from it; and when not insulated, every other conductor with which it is connected takes a part in proportion to their conducting property. As the zinc disc is found to be much stronger when the copper disc is not insulated, I conclude that zinc has the property of attracting the electric fluid out of copper when they are in contact.

The effect is stronger when the zinc alone is insulated, and less when the copper alone is insulated; hence the zinc is inferred to *attract*.

In regard to the second experiment, where the zinc is found to be much more strongly positive than in the first, is clear from what I have already said, *i. e.* because the copper disc is not insulated, but is free to act as their mutual action upon each other may require.

In expt. II. the insulation of the zinc increases the effect.

In the third experiment, in which no effect should follow (according to Sig. Volta), the zinc was found to be as strongly electrified as in the last experiment. Neither the zinc nor the copper being completely insulated, they are at liberty to act upon each other as their mutual contact directs; the zinc at liberty to attract and the copper to give, which is in no wise repugnant to the old laws of electricity.

Expt. III. The metals being at liberty to assume the respective states on contact, did so accordingly.

How Sig. Volta could be led to such erroneous conclusions is not easy to understand, unless he was deceived by the condensers he made use of. "To render that feeble electricity "sensible and manifest, he recommends flat metallic plates co- "vered with a slight layer of sealing wax or lac varnish," which I was not a little surprised at, because I have always found such condensers very equivocal in their results, and shew different signs without any variation or obvious cause, or at least such as I was unable to detect. They are more easily excited by a negative power than by a positive one, and retain it much more tenaciously; and hence, when they happen by any means to be strongly electrified by a negative power, it is almost impossible to discharge them of it, so as to be fit to proceed on immediately with experiments that require nice investigation.

Sig. Volta supposed to have been deceived by his *varnished condensers*.

Since the invention of the galvanic instrument, various electrometers, condensers, doublers, multipliers, &c. &c. have been used to investigate its electrical properties, all of which appear

Mr. John Read's construction of the condenser.

appear to me to be much inferior to one conftructed in the year 1796, by Mr. John Read, an ingenious electrician at Knightfbridge; but as he invented it near the time of his retiring from bufinefs, he did not publifh it, confequently it is known but by very few electricians. I have found it of great ufe not only in galvanifm, but in all experiments wherein fmall quantities of electricity are to be made evident; and I doubt not but that it will be confidered as a valuable acquifition by electricians. I have found it capable of rendering much fmaller quantities fenfible than any other inftrument. By this inftrument we are able to learn the pofitive and negative fides of only one piece of *zinc, copper, and wet cloth;* and I have not heard that it has been done by any other inftrument lefs than with a feries of twenty*.

A Defcription and Ufe of Mr. Read's compound Electrical Condenfer.

Defcription of Read's condenfer.

Fig. 1. plate XVI. reprefents a vertical fection of the large condenfer: *a a* is a round flat plate of brafs of about eight inches, ftanding infulated upon a wooden foot *g*; *g h* is a hollow brafs cylinder, and *f h* is a folid ftick of glafs, with a brafs mounting at *f*, to which the plate *a a* is fixed; *b b* is another plate of brafs fomewhat lefs in diameter, and has a round hole in the middle of about two inches diameter; *c d* is a hollow cone fixed to *b b* at *c c*; *d e* is a hollow brafs cylinder fixed to the cone at *d*, and made to flide up and down upon the cylinder *g h*; *i* is a milled head-fcrew which holds *b b* faft when it is at its proper diftance from *a a*, where a ftop is made for that purpofe; when *i* is loofe, *b b* falls down by its own weight, and refts upon the foot *g*.

Altered by the author.

The above is the original conftruction, which I found more complex and lefs portable than I could wifh; for which reafons I make them in the following manner; and fixing the condenfing plates in a vertical pofition inftead of horizontal, it has all the advantages, is more fimple, and perfectly portable, and I have no doubt will meet with the approbation of Mr. Read himfelf.

Defcription of the improved Condenfer.

Defcription of the improved condenfer.

Fig. 2 reprefents a vertical fection of the large condenfer ftanding edgewife to the eye: *a a* and *b b* are two flat round

* Bennet's electrometer alone fhewed the ftate of 40 pairs. See Van Marum in Philof. Journal, octavo, I. 174.—N.

brafs

brafs plates of about fix inches; *c* is a glafs cylinder fixed at one end into a wooden foot *d*; *e* is a brafs ball, and mounting at the other end to which the plate *b b* is fcrewed faft; *f* is a brafs wire with a joint at its lower end, and at the other end a ball to which the plate *a a* is fcrewed, ftanding parallel to *b b*. By means of the joint, the plate *a a* may be moved back in the fituation of the dotted outline *a g*. The joint has a fhoulder which ftops the plate *a a*, and keeps it at its proper diftance from *b b*.

Bennet's electrometer with a condenfer.

Fig. 3 is the common gold-leaf electrometer, with an addition of two vertical brafs plates of about $1\frac{1}{2}$ inch in diameter; one is fcrewed faft to the brafs mounting at the top of the electrometer, and the other to a wire which has a joint fixed to the foot of the electrometer, by which it moveable, and is fet either parallel and at a proper diftance from the other plate, the joint being furnifhed with a ftop for that purpofe, or may be moved backwards in the direction of *g a*, fig. 2: *l* is a brafs cup with a fhank at the bottom, which fcrews into a hole in the top of *e*, fig. 2, ferving to examine the ftate of electricity excited by dropping of metals, &c.; *m* is a ftick covered with tinfoil, which fcrews into the hole at *e*, to examine the electricity of the atmofphere; *n* is a brafs wire jointed at *o*, with a fhank, which can alfo be fcrewed into *e* when required, or into the top of the gold leaf electrometer, fig. 3. Thefe two inftruments, fig. 2 and 3, are ufed feparately or combined, as the nature of the experiment may require. When the experiment requires the aid of both condenfers, they are combined, as is reprefented fig. 4, the two fixed plates ftanding towards the eye. The fixed plate of the large condenfer has a fmall brafs pin at one fide, which, when the inftruments are ufed together, muft touch the fixed condenfing plate of the gold-leaf electrometer *.

THE METHOD OF USING THE DOUBLE ELECTRICAL CONDENSER.

To fhew the Electric Fluid excited by Effervefcence, &c.

Ufe of the two condenfers. To fhew the electricity from effervefcence, &c.

Screw *l* into the top of the ball *e* of the large condenfer, and fet therein a china or glafs cup with proper ingredients for that purpofe;

* Inftead of gold leaf Mr. Read ufes very fine fibres of flax, which he thinks more fenfible than gold leaf; but they are very difficult to be feen, and more eafily deranged. If gold leaf be properly managed, I think it preferable.—C.

purpofe; then join the two inftruments as in fig. 4. While the effervefcence is going on, turn back the moveable plate of the large condenfer into the pofition of the dotted line, fig. 2, taking care not to touch the fixed plate; then if the excited electricity be very ftrong, the gold leaf will diverge; but if not, juft move the electrometer fo that it is quite free from the pin, turn back its moveable plate, and if a fufficient quantity of electric fluid be excited, the gold leaf will diverge.

To fhew if there be any fenfible Quantity of Electric Fluid in the Atmofphere.

Atmofpheric electricity.

Screw the ftick *m* into *e* of the large condenfer; a convenient place being chofen not much furrounded by buildings or trees, fet the inftruments combined as in the laft experiment, and proceed in the fame manner.

Method of applying the combined Condenfers to the Galvanic Inftrument.

Method of applying the condenfer to the galvanic inftrument.

Screw the fhort end of *n o* into *e* of the large condenfing plate (the inftruments being combined as in the former experiments); bend the end downwards, at fuch a diftance from the table, or whatever it may ftand upon, that the two pieces of metal, zinc and copper, as at *n*, can be put under it, and drawn away from under it again, without its touching the table when the metal is drawn away. Take two pieces of metal, zinc and copper, about the fize of half a crown or upwards, either feparate or foldered together, with their flat fides in contact, and pufh them under the end *n* of *o n*. After remaining a fhort fpace of time, a quarter or half a minute, draw them away from under the point, and take notice that the point does not touch the table, or any other conductor; then turn back the moveable plate of the large condenfer; move the electrometer fo that its plate fhall no longer touch the pin of the large plate, and then turn its moveable plate back; the gold leaf will remain undifturbed.

Electricity from the galvanifm of a fingle pair of plates.

Turn up the condenfing plates to their firft pofition; place the two inftruments together as before, taking particular care that the fixed plate of the electrometer condenfer touches the pin proceeding from the large plate. Lay upon the pieces of metal before ufed, a piece of woollen cloth well foaked in a folution of muriate of ammonia, or any other menftruum commonly

monly ufed for the galvanic experiments either upon the zinc or the copper, and pufh them under the point of *n* again. Prefs the point down upon them, that it may be perfectly in contact; after they have remained the time before-mentioned, draw the metals away, and feparate or turn back the large condenfer plate, and alfo the fmall one, after feparating it from the pin of the large one, and immediately the gold leaf will diverge. If the zinc was the uppermoft, then the gold leaf will diverge with pofitive electricity; but if it was underneath, the gold leaf will diverge with negative electricity. It makes no difference in the general effect, upon which metal the wet cloth was laid; or whether two pieces of cloth were ufed, one under the metals and the other above; or only one either above the metals or under them. But if the cloth be only laid upon the copper and not upon the zinc, the electric fluid brought into action will be fo weak, that the combined inftrument can hardly fhew it; if laid upon zinc, the divergency will be about $\frac{1}{20}$ of an inch; fometimes more and fometimes lefs *.

By reflecting on this phenomenon I found the following explanation, without having recourfe to any new hypothefis.

Explanation of the Action of the Electric Fluid in the Galvanic

Explanation of the facts offered. Zinc and copper in contact arrive at an equilibrium of electricity; the zinc + and the copper —.

When flat pieces of zinc and copper are laid in contact, the zinc becomes pofitive and the copper negative at the moment of the touch; and while they remain in contact, the electric fluid contained in them both is perfectly in equilibrio. The copper has given and the zinc has received fuch a quantity of electric fluid as their mutual action upon each other required; and in confequence of this property, they prefent a mutual refiftance to any further change being produced upon them. If then any menftruum be added to the oppofite fide of the metals, capable of producing a change in their metallic property (fuch as the fluid contained in the wet cloth), a change in their electrical property muft of courfe follow. But as this change in the metallic property is only fuperficial, it will only be there that its electric property is changed. The other parts of the two metals in contact will remain unaltered, and maintain their property of refiftance. The change produced by the action of the

Chemical action alters the electrical property of that part of the metal in which it takes place.

* When the atmofphere is in a favourable ftate. In this as well as all other experiments where fo fmall quantities are to be made evident, the atmofphere has great influence.

the

This change is *asserted to be* of an opposite nature to the electric state of the unaltered metal, and therefore produces electric states at the wet surfaces opposite to those of the dry metals: which do not pass from zinc to copper *through the metals* and become quiescent; but from zinc to copper *through the fluid*, this passage being easiest. Hence the galvanic current: which is progressive, because the fluid cannot perfectly conduct the electricity as fast as it is extricated.

the menstruum, with respect to its electrical property upon that part only of the metal whereon it has acted, is exactly the reverse of that of the parts not acted upon. The part of the zinc thus acted upon, must consequently be disposed to throw off its electric fluid, and would give to that part of the copper, which by a like action is disposed to absorb it) so that the two states of the surfaces acted upon would unite and counterbalance each other imperceptibly) if it were not opposed by the still existing property and mutual action of the two metals in their parts not acted upon by the menstruum; hence it follows that the electric fluid is propelled forwards from the zinc *through the menstruum* to the next adjoining copper in the pile or trough; but this can only happen in a progressive manner, on account of the menstruum being an imperfect conductor, which appears to be an indispensible condition to the maintaining any electric intensity.

The shock from the accumulation suddenly transmitted.

The shock or sensation felt on touching the two opposite ends of the galvanic instrument, depends upon the menstruum, together with the resisting property of the two metals in contact. The fluid must be adequately proportioned between being an electric and a perfect conductor. If it were a perfect conductor, the electric fluid would pass from the zinc through the menstruum to the next adjoining copper, as quick as it is given off by the altered part of the zinc; no accumulation would ensue, and consequently no sensation of shock or discharge would be perceived. If it were an electric, the electric fluid given off by the altered zinc would be stopped, and accumulate till it become possessed of sufficient force to overcome the mutual resistance of the two metals in contact, and pass through by a reverse motion to the copper; consequently no sensation would be felt by forming a communication between the two opposite ends. For though there would be an accumulation, yet it cannot be united so as to act in concert with that of the other combined metals; being shut off by the interposed electric menstruum, and too feeble of itself to affect our senses.

ANNOTATION. W. N.

Short history of instruments for shewing minute quantities of electricity. W. N.

At page 396, vol. I. of the quarto series of this Journal, an outline is given of the history of all the instruments for shewing or measuring minute quantities of electricity, with a statement of their advantages and defects. With regard to the condenser discovered

difcovered by Volta in 1780 *, and ufed for atmofpheric electricity and in the grand difcovery of the electricity produced by chemical changes, it was applied very early by Cavallo in the compound form; that is to fay, by charging a fmaller inftrument with the contents of a larger. Mr. Bennet before 1787 †, added the condenfer to his electrometer, and invented the method called 'oubling, which had before been ufed with the electrophore by Lichtenberg ‡ and Klincock §. Our electricians, particularly Mr. Cavallo and Mr. Bennet, were at this time fully aware of the fpontaneous electricity of the doubler, and the evils to be apprehended from the varnifh and the actual contacts. I think it was Mr. Cavallo who fubftituted fmall knobs of fealing wax inftead of varnifh, which I faw adopted in a mechanical doubler by Dr. Darwin in 1787; and in the year 1788 I communicated to the Royal Society || the revolving doubler, in which the plates approach and recede without touching; and foon afterwards I made the fpinning inftrument, confifting of a condenfer and an electrical well. In the fame year Mr. Cavallo publifhed ** his *collector* of electricity, which is a condenfer having two uninfulated plates, which feparate like plate *a*, fig. 2, on each fide of *b*, without contact; and in the third vol. of his "Complete Treatife on "Electricity," 1795, he defcribes his *multiplier*, which confifts of two condenfers, equally perfect; one of which is made to charge the other by repeated alternations of the compenfating plate.

The above fhort fketch will enable the reader to afcertain to what extent the ingenious contrivers of the inftruments in plate XV. have availed themfelves of the labours of former operators.

* Journal de Phyfique for May and Auguft 1783, and Phil. Tranf. LXXII. p. 237.

† Phil. Tranf. LXXII. p. 32.

‡ Journal de Phyf. Jan. 1780, p. 20.

§ Phil. Tranf. LXVIII. p. 1029.

|| Phil. Tranf. LXXVIII.

** Phil. Tranf. LXXVIII.

XV.

Observations on the Phosphorescence of the Tremolite, and of the calcareous Phosphate of slow Solution, known by the Name of Dolomie. By M. LE COMTE DE BOURNON, *Fellow of the Royal and Linnæan Societies. Translated from the Original; communicated by the Author.*

The phosphorescence of minerals has been little inquired into.

FEW researches have yet been made concerning the phosphorescence of the bodies of the mineral kingdom. No satisfactory explanation has yet been given of this phenomenon, though this knowledge would undoubtedly throw new light upon the study of minerals, and prove a great acquisition to natural philosophy and chemistry.

The methods of exciting it: 1. by friction; 2. by heat.

This property of emitting light, which daily observation shews to belong to many more minerals than had formerly been suspected, requires particular management. In some fossils, such as quartz, blende, corundum, &c. &c. it becomes sensible only by friction. In others it exhibits itself only when the mineral is placed upon a red hot coal, or upon any other body heated to a similar temperature. This is the case with the carbonate of strontian, of barytes, &c. In others again, the phosphorescence is developed both by friction and by heat, as is the case with the phosphates and fluates of lime, as well as with a great number of carbonates of the same earth, particularly those of a brown or yellowish colour.

These facts give occasionfor several questions, the solution of which would be extremely interesting.

Does it arise from combined light?

Do these two kinds of phosphorescence depend upon the same cause? In all the stones which exhibit them, and are at the same time coloured, the colour diminishes in proportion to the disengagement of the phosphorescence by the action of heat: and when they cease to be phosphorescent, they at the same time intirely cease to be coloured. Does this phenomenon proceed from the disengagement of the combined or interposed light? Does the colour in these stones always belong, in reality, to metallic oxides, particularly those of iron? May it not rather belong simply to the combined light? In this case, may it not be supposed that the light is decomposed, by combining with these stones, and that it then entered into their

composition only by insulated rays, or by combination of two or more, and not the whole of the rays? We are, however, enabled to make an observation with respect to this subject, namely, that these stones constantly exhibit by the action of heat, a phosphorescence of the same colour, whatever may be the colour of their proper substance. For instance, the fluates of lime which exhibit the most lively and variegated colours, constantly give a light inclining to the violet, with the single exception of the Siberian variety, which has been named the Chlorophane, and which, though of a violet colour, gives a phosphorescence of a beautiful emerald green. In others, as in some carbonates of lime, in those of barytes, of strontian, &c. though these stones are perfectly colourless, the phosphorescence is constantly reddish, or orange yellow. What may be the cause of these contrasts?

The phosphoric colour is the same in all stones of the same species, however themselves may be coloured.

In some cases the cause which produces the phosphorescence of the stones seems to belong to an essential part of their substance, which is never completely expelled from them. This is the case with the calcareous fluates and phosphates, &c. In others it appears to be purely accidental in the stone, and shews itself only in a certain number of individuals belonging to the same species. In the first case, this property ought to be indicated amongst the specific characters of the stone; in the second, it cannot serve as character of the species, but can merely be used to designate one of its varieties. Such is, for example, that which exists in the tremolite and in the dolomie, respecting which I intend here to offer some observations, which appear to me to deserve the attention of mineralogists.

It may arise from a permanent cause, or from some portion that can be abstracted.

M. de Saussure and Professor Blumenbach were, as far as I know, the first who observed the two kinds of phosphorescence in the tremolite; and, since them, all the works of mineralogy have placed this property amongst the distinctive characters of this stone. Many tremolites, indeed, are endowed with this double phosphorescence; but this is by no means the case with all, nor can this character be considered as essential to its nature.

The tremolite is not in all specimens phosphorescent.

The tremolite, both that which is found in different vallies of Mount St. Gothard, and that which is brought to us from a great number of other places, is generally inclosed in a granulated carbonate of lime, the grains of which are of various degrees of fineness, and their adhesion more or less considerable.

Those which are in a phosphorescent gangue are themselves phosphorescent; and on the contrary when the gangue is not phospho-

refcent the tremolite is not fo.

Among thefe carbonates of lime, which conftitute the gangue of the tremolite, and very frequently belong to the fpecies which is called dolomie, a great number are endowed with the double phofphorefcence; but, on the other hand, we meet with feveral, in which not the flighteft trace of this property can be found. The tremolites inclofed in the firft partake, though in rather an inferior degree, of their phofphorefcence, whereas thofe which are inclofed in a non-phofphorefcent carbonate of lime are equally void of that property.

That the phofphorefcence arofe from interpofed carbonate of lime, was proved by taking it up by nitric acid, which deftroyed the luminous quality.

From the very firft moment when I obferved this fact, it occurred to me, that the phofphorefcence of the tremolite might very probably proceed merely from that of the carbonate of lime, which undoubtedly is interpofed between its parts. I therefore felected fome cryftals that were inclofed in phofphorefcent carbonate of lime, and after having fatisfied myfelf that they were themfelves phofphorefcent, both by friction and by the action of heat, I digefted them for fome time in nitric acid. When I took them out, their furface was perforated with fmall cavities, occafioned by the folution of the portions of carbonate of lime, and friction was then incapable of making them difplay the flighteft phofphorefcent light. This light, however, was emitted, at the point immediately after ignition, though in an extremely weakened degree. I afterwards reduced fome of the fame cryftals to a coarfe powder, and this powder having remained again for fome time in the acid, was intirely deprived of all its phofphorefcent property.

This quality therefore belongs to the variety, and not the fpecies.

I could no longer doubt that the carbonate of lime, interpofed between the particles of the tremolite was the real caufe of the phofphorefcence of this fubftance, when its gangue was endowed with the fame property. It therefore appeared to me to be at the fame time perfectly proved, that phofphorefcence cannot be ranked amongft the characters of the fpecies in this fubftance, and that it ought to be confidered merely as diftinctive of one of its varieties.

Whether the large portion of lime faid to exift in tremolite can be admitted *as a conftituent part?*

A new doubt, which was a very natural confequence of this obfervation, now prefented itfelf. Could it be true, that the lime which chemifts have reckoned to amount to $\frac{15}{100}$ amongft the conftituent parts of the tremolite, does exift in it in fo large a proportion. In order, if poffible, to fatisfy this doubt, I felected fome cryftals of non-phofphorefcent tremolite, which

had only an argillo-martial fubftance for their gangue, and requefted Mr. Chenevix, who has already rendered fuch ufeful fervices to mineralogy, to examine them by analyfis. I likewife gave him fome cryftals from amongft thofe phofphorefcent ones which I had broken, and afterwards deprived of their phofphorefcence, by digefting them for fome time in nitric acid.

Probability that a portion of the gangue, whether lime or argil, is *interfperfed* in the mineral.

My fufpicions were verified: Mr. Chenevix found only $\frac{4}{100}$ of lime in each of thefe two analyfes. But what at the fame time ftruck me, was that the tremolite taken from the phofphorefcent variety, having for its gangue a carbonate of lime of the fpecies called the dolomie, which is likewife phofphorefcent, but whofe calcareous part had been taken away by the nitric acid, gave by analyfis only $\frac{4}{100}$ of argil, whilft that which was taken from an abfolutely argillaceous gangue gave $\frac{14}{100}$ of the fame earth. Mr. Klaproth having found no argil at all in the analyfis which he had before made of this fubftance, it is probable that its exiftence in it, like that of the calcareous carbonate, proceeds merely from its fimple interpofition.

Hence it is of importance that chemical analyfes of any mineral fhould be repeated upon fpecimens taken from different gangues.

Thefe two obfervations appeared to me to be very interefting to the ftudy of mineralogy, efpecially to that part of it which relates to analyfis; fince it fhews with what care the chemift ought to avoid confounding, with the true conftituent parts of a fubftance, thofe which are foreign to it, and only interpofed between its parts. It is extremely common to find the parts of a mineral, even in the ftate of cryftallization, envelope more or lefs of the portions of that fubftance which conftitutes its gangue; and what may likewife contribute much to miftakes in this refpect, is the kind of conftancy with which, (whatever may be its caufe, has hitherto been little attended to) the fame fubftance, placed in fimilar circumftances, admits this interpofed extraneous fubftance, in equal, or nearly equal proportions. It is not therefore fufficient that the chemift fhould felect for his analyfis, amongft the cryftals of a fubftance, thofe which appear to him to be the moft pure, (and the perfection of their form and tranfparency is the ftrongeft prefumption which he can have in this refpect;) but he fhould alfo repeat the fame analytical procefs upon the fame fubftance taken from totally different gangues.

When

The phosphorescence of tremolite being admitted to arise from interspersed carbonate of lime, will account for the differing observations of authors.

When it is once admitted that the tremolite is phosphorescent only in proportion as this property is contained in the carbonate of lime interposed in its substance, the variations which some authors have found in its phosphorescence, will be easily accounted for. It must, for example, be the more easily obtained by friction, as the hardness of this stone is less considerable; because the friction, by breaking its surface, will successively arrive at the interposed particles of carbonate of lime. And hence it is quite natural that the fibrous varieties should be more phosphorescent than the others, and that these should be the less so in proportion as they are harder.

Though the phosphorescence of tremolite cannot be admitted as specific, yet the facility with which this very hard mineral is crushed by pressure with the hammer and its elastic resistance, are peculiar specific characters.

As this character, which was represented as specific in the tremolite, must no longer be considered as such, there is one which has been overlooked, and which, in my opinion, ought to be added to those which have been already observed in it; I mean the great facility with which, notwithstanding its hardness, which, in the purest specimens is such that it easily cuts glass, it is crushed by the mere pressure of the hammer, and the kind of flexibility which it then exhibits. If, in breaking it, the pressure be moderate, the crystals of tremolite divide, pretty generally, according to the length of their prisms, into small fibres, which are frequently as fine as those of the amianthus, to which, in this state, they have much resemblance. We may then even increase the pressure without breaking the fibres, which in this case afford by their resistance the same sensation as is felt from a slightly elastic body. This effect, as well as the reduction of the tremolites into small fibres, is more distinct the less pure the mineral. Both properties however are observed, but in a much less degree in the purest tremolites, and consequently in those whose hardness is the most considerable.

Tremolite is found in Scotland, at Mount Vesuvius, and in Bengal.

With a view to add as much as possible to the knowledge already acquired respecting this substance, I shall add to the list of places which have been indicated as the native countries of the tremolite, Scotland, Mount Vesuvius, and Bengal. Mr. Greville's rich cabinet in London contains specimens from each of those different places; a description of these will probably be acceptable to mineralogists.

Description of the tremolite of Scotland.

The tremolite of Scotland is in the fibrous state, its fibres being very fine and close, part of which is disposed in divergent

gent rays, tranfverfely croffed by other fibres, fo as to reprefent a kind of texture, as is fometimes the cafe with the zeolite mezotipe. This tremolite is of a greenifh white colour: it adheres to a granulated but very compact mafs of carbonate of lime, mixed in almoft equal proportions with the fame tremolite, alfo in a granulated ftate, on which account it ftrikes fire with fteel. This carbonate of lime poffeffes both kinds of phofphorefcence, but the light which it gives is of a flightly blueifh colour; the fame is the cafe with the tremolite which it contains. This carbonate does not belong to the dolomie, and it diffolves in acids in the fame manner as the common carbonate of lime.

The tremolite of Vefuvius is likewife in the fibrous ftate, with fine and clofe fibres; its colour is a greyifh white, it adheres to a gangue compofed of carbonate of lime, of an immenfe quantity of very fine fmall fibres of the fame tremolite, and of a great number of very fmall cryftals of pyroxene, of a beautiful green colour and tranfparent. Some portions of idocrafe are alfo obfervable, of which there is a group at one of the extremities of the fpecimen in pretty large cryftals. The carbonate of lime does not belong to that of flow folution; it poffeffes both kinds of phofphorefcence, and the light which it emits is of a very lively deep orange colour. The tremolite exhibits abfolutely the fame phofphorefcence. Tremolite of Mount Vefuvius.

We owe our knowledge of the tremolite of Bengal to Sir John Murray. It is in pretty large cryftals of a greenifh grey colour, bedded feparately in a granulated carbonate of lime, the very fine grains of which have a ftrong adhefion to each other; a character which, joined to the great whitenefs of this ftone, caufes it very much to refemble a piece of double refined fugar. This carbonate of lime belongs to the fpecies of the dolomie; it is even one of thofe in which I have obferved the folution to be the floweft and moft infenfible, but it is neverthelefs completely diffolved in the nitric acid, leaving only a light whitifh and clouded refidue, which difappears when the acid is diluted with water. Its hardnefs, which is much fuperior to that of the ordinary carbonate of lime, is rather inferior to that of the fluate of the fame earth; and this is the cafe with all the dolomies, not excepting thofe whofe grains have the leaft adhefion with each other. This dolomie is not phofpho- Tremolite of Bengal.

phosphorescent either by friction or by the action of heat, and the tremolite which it contains is intirely in the same state. There are many other dolomies also which possess no phosphorescence, as has been also observed by the Abbé Hauy. This character therefore still belongs to the variety, and not to the species.

XVI.

Outline of the History of Galvanism: with a Theory of the Action of the Galvanic Apparatus. By JOHN BOSTOCK, *M. D.* *From the Author.*

To Mr. NICHOLSON.

SIR,

Introductory letter.

HAVING been lately employed in some experimental inquiries on the subject of galvanism, I found it commodious to arrange the numerous discoveries that have been made in this department of science into the historical form. The facts which have been successively developed begin to assume so elevated a rank among the branches of natural philosophy, that a sketch of the most important and best established amongst them seems desirable, in order that the experimenter may be easily enabled to see what has been done by his predecessors, and may thus be prevented from wasting his time and exertions upon points which have been previously investigated. From reflecting upon the labours of others, and comparing them with my own experiments, I have been led to form a theory of the action of the galvanic pile, which seems to explain in an easy manner most of its singular properties. I am indeed well aware of the undue attachment which every one feels for the offspring of his own imagination, and I shall not be surprized if you perceive in the hypothesis many blemishes which have escaped my notice. I have however sent you both the history and the theory, in order that you may insert them in your Journal, if you think them deserving of a place there.

I am, SIR,
Your obedient Servant,
JOHN BOSTOCK, *M. D.*

Liverpool, June 1, 1802.

HISTORIC

HISTORIC SKETCH OF GALVANISM.

THE firſt publication * upon the ſubject is the work of Galvani himſelf, which appeared in 1791. It begins by giving an account of the following accidental diſcovery. A frog had its hinder legs ſeparated from its body, except that the crural nerves were left undivided, and was by chance laid upon a table on which ſtood an electrical machine. It was obſerved that when the animal was placed in contact with, or ſufficiently near to, an extenſive ſurface of a conductor of the electric fluid, if a ſpark were taken from the machine in any direction, the legs of the animal were ſpaſmodically contracted. When the frog formed part of the electric circle, ſo that the fluid paſſed immediately through it, a quantity almoſt imperceptible was found to excite contractions of the muſcles. A much ſmaller quantity of the electric fluid produced the effect, when the animal was prepared in the manner deſcribed above, than when the body was left intire, becauſe in the former caſe the fluid was confined to the narrow track of the nerves in its paſſage along the circuit. A *prepared frog* appears therefore to be a moſt delicate electrometer, as it exhibited contractions where no marks of electricity could be diſcovered by the inſtruments either of Bennet or Cavallo. Galvani afterwards found that contractions could be produced in the limbs of prepared frogs by the electricity of the atmoſphere, and it was in conſequence of ſome arrangements which he made for this purpoſe, that he was led to his great diſcovery of animal electricity. He found that he was able to produce contractions in the limbs of frogs without the aid of any foreign or artificial excitement, merely by the application of a conducting ſubſtance from the nerve to the muſcle. Theſe contractions were capable of being produced, of whatever ſubſtances the circuit of communication was compoſed, provided only they were all conductors of the electric fluid. The effects were found to be much increaſed by applying a metallic coating to the nerve. He found this peculiar ſpecies of electricity to exiſt in a great variety of animals, and that the contractions may be excited

First publication on galvaniſm by *Galvani* in 1791. Legs of a frog, ſeparated from the body, were convulſed by extremely minute portions of electricity.

Proper *galvaniſm*, or contractions without excited electricity.

* Sultzer in his Theorie des Plaiſirs, quoted by Fabbroni, Philoſ. Journal, quarto IV. 120, mentioned the taſte by contact of two metals.—N.

either

either in the whole body or in particular parts of it, as long as the animal possesses any remains of vitality. Galvani supposed these phenomena to be analogous to the effects of the Leyden phial; that there is an excess of electric fluid in the interior of the muscle or in the nerve, and a deficiency on the outside, or *v v.* The nerve he conceived to act the part of the wire in the Leyden phial. Soon after the publication of Galvani's work, Valli's Letters on Animal Electricity appeared in the Journal de Physique, vol. 41. & seq.

Theory of Galvani; that the interior of the muscle is charged plus, and the nerves form a communication with the outside.

Valli's Letters on Animal Electricity, 1793.

In the Transactions of the Royal Society for 1796, Volta's letter to Cavallo was published, which besides giving an account of Galvani's discoveries, contains many original experiments and observations. The analogy of the Leyden phial he shows is without foundation, for he found that he could excite similar contractions in the limbs, when the conducting circuit only touched two parts of the nerve, two muscles, or two parts of the same muscle; in order to accomplish this, it is however necessary to use two different metals. He supposes that in these cases the muscular contractions are produced by a small quantity of electricity which is excited by the action of the metals upon each other; this he conceives to depend upon a general law of the electric fluid, and that its effects are visible in the experiments of Galvani only because the prepared animal is the most delicate species of electrometer. Volta endeavours to prove by experiment, that the action is always in the first instance upon the nerves, and that the muscles are only affected through their medium. He imagines that it is not necessary that a communication should exist between the nerves and the muscles according to the opinion of Galvani, he imagines that the contractions will be produced in the limbs, if the influence be only made to pass from one part of a nerve to another part of the same. But we shall find that in this idea Volta is probably mistaken, as in his experiments the moisture adhering to the nerve formed a communication between it and the muscle. He found that if different parts of a nerve, or indeed if the body of the animal in general, be laid upon two different metals, and these metals be made to communicate by a conducting substance, muscular contractions are produced. These experiments succeed with more certainty when the skin is removed; this precaution is more especially requisite if the animal have a dry skin, as is the case with birds and quadrupeds.

Volta's Letters to Cavallo, 1793.

No charge in the animal.

but electricity from the contact of different metals applied to the nerves.

The galv. action supposed to exist in nerves only.

peds. In a fecond letter which is printed in the fame vol. of the Phil. Tranf. Volta purfues his experiments and obfervations upon the fubject of galvanifm. If a fingle mufcle, or a part of it be armed with coatings of two different metals, and thefe be made to communicate by a conductor, the contractions will be excited, but no effect will be produced if two coatings of the fame metal be employed. Worms and fnails he found could not be excited by this influence; but flies, beetles, grafshoppers, and butterflies, he found were fubject to its action. Upon the whole it appears that thofe animals only which have diftinct limbs, with flexor and extenfor mufcles, are excitable by animal electricity. In thofe animals which are acted upon by the galvanic influence, Volta found that it was only the mufcles which are under the direction of the will that can be made to retract; in his experiments he never perceived that the heart was affected by the two metals, though this organ is thrown into ftrong contractions by the flighteft chemical or mechanical ftimulus. The two metals which were found by their union to perform the moft powerful effects were zinc and filver. Volta placed thefe metals one on each furface of the end of the tongue; when they were brought into contact no motion was produced, but a ftrong fenfation of tafte was excited. When the metals were applied to the root of a tongue cut from the mouth, contractions were produced.

Of the various animals, thofe only were found to be affected which have diftinct limbs, &c. and thofe mufcles only which are fubject to the will.

Zinc and filver.

In the fame year in which Volta's letters appeared in the Phil. Tranfactions, Fowler publifhed an effay on Animal Electricity; thefe works muft therefore be confidered as equally original. The extracts given above from Volta's letters, prove that he confidered the phenomena of galvanifm to depend upon the operations of the electric fluid; other experimenters however conceived it impoffible to reconcile the new difcoveries with their previous ideas of the nature of electricity, and Fowler commences his treatife by this enquiry. In order to afcertain this point he inveftigates the circumftances which are neceffary to the production of the mufcular contractions. Thefe he found to be the contact of the two metals with each other, and their communication with the animal; the contractions may alfo be produced by bringing the metals in contact with each other in water, without either of the metals touching the animal. When the metals are applied to the

Fowler's Effay, 1793. Whether galvanifm be electricity.

Conditions of the effect: that two different metals fhould touch each other and the animal; viz. the nerve and mufcle;

either immediately, or mediately by water.

the nerve alone, Fowler ſtill conſiders it requiſite that there be a connection between the nerve and the muſcle; in ordinary caſes this connection is effected by the moiſture which adheres to the nerve, a circumſtance which Volta ſeems to have overlooked. Valli had endeavoured to form a theory of animal electricity founded upon the idea that the electric fluid was unequally diſperſed through the body, and that the application of the metals produced an equilibrium; Fowler performed a number of experiments to diſprove this idea, and apparently with ſucceſs. He concludes the firſt part of his eſſay by giving it as his opinion, that the phenomena of galvaniſm are not reconcileable to the known laws of electricity, becauſe for the excitement of the electric fluid, motion between an electric and a conductor appears to be neceſſary, whereas in theſe experiments two metals only are the ſubſtances employed. He alſo failed in cauſing charcoal to conduct this influence, though it is a better conductor of electricity than the animal fluids; in this particular, however, he appears to have been miſtaken. He conſiders the influence as very ſimilar in its effects to the action of the torpedo, though not altogether the ſame. He was unable to produce the muſcular contractions in worms, yet he found that when a worm or a leech lying upon ſilver, put its mouth upon zinc, it appeared to ſuffer great uneaſineſs. Dr. Fowler in the ſecond place proceeds to enquire, whether all the muſcles of the body be ſubject to this new influence. He found it difficult to excite any contractions in the heart, though at length, by uſing proper precautions, he ſucceeded; but he was not able to excite contractions in the ſtomach or inteſtines. He diſcovered that when the nerves of viſion were acted upon by the two metals, a ſenſation of a flaſh of light was perceived in the eye. When inflammation was excited in a limb, it appeared to acquire additional ſenſibility to the galvanic influence. Fowler's work concludes with a letter from Profeſſor Robiſon, who made ſome new obſervations upon the production of the galvanic flaſh, and firſt noticed the effect produced by applying the tongue to a number of pieces of ſilver and zinc alternately piled upon each other.

Attempt to diſprove the theory of an electric charge in the animal ſyſtem; and to ſhew that galvaniſm is not electricity.

Torpedo.

Effect on worms, and on the involuntary muſcles.

Diſcovery of the flaſh of light.

Profeſſor Robiſon makes a pile of zinc and ſilver.

Darwin in 1794 conſiders galvaniſm as electricity.

In 1794 the firſt volume of the Zoonomia was publiſhed; Darwin ſpeaks of the phenomena of galvaniſm, and conſiders them as electrical. The muſcular contractions he ſuppoſes depend

depend upon the fenfibility of our nerves to fmall quantities of the electric fluid. Bennet difcovered by means of his electrometer, that zinc, when feparate from other metals, always is in the minus ftate, and filver in the plus ftate; when therefore they are brought nearly together, a fmall plate of air is charged like a Leyden phial; when the metals are brought into contact this is difcharged.

Bennet's early difcovery of the plus ftate of zinc (1789.)

In the Phil. Tranf. for the year 1795, is a Paper by Dr. Wells on the fubject of galvanifm. He propofes for confideration the three following enquiries; Do the contractions obferved by Galvani depend upon any property peculiar to the living body? What are the conditions neceffary for the excitement of the influence? Is it electrical? With refpect to the firft queftion, he fuppofes that animals act only on account of their moifture. With refpect to the fecond, he found that one metal and charcoal excited the contractions as readily as two metals; in this he corrects the miftake of Fowler noticed above. He however found that all charcoal will not act as a conductor of the influence, in which opinion Volta agrees with him. Wells does not agree in the hypothefis adopted by Volta and others, that the contact of the metals produces an alteration in the difpofition of the electric fluid, for he very properly afks, why fhould not the natural moifture of the animal afford a communication between the two metals before they are connected by any other conductor? He farther difcovered, that contractions could be excited by one metal only when it had been rubbed upon another metal, or even upon the hand. Charcoal may by the fame means be made to produce the fame effect; he proved by a variety of experiments that the friction does not in thefe cafes communicate electricity to the metal or the charcoal; we may conjecture that an incipient oxidation, or fome other chemical change was produced upon the furface of thefe fubftances. With refpect to the third queftion, Wells is decidedly of opinion, that the phenomena are electrical; the influence being conducted by all conductors of electricity, and by them alone.

Dr. Wells, 1795, ftated from experiments that animals are affected only by means of humidity; that the ftate of the metals is not altered by contact; but that the phenomena are electrical.

Befides the works here mentioned a number of communications appeared in the different fcientific journals of Italy, France, and Germany, and feveral diftinct treatifes were publifhed on the fubject of animal electricity. Humbolt particularly

Various other papers, Humboldt, Dr. Monro, the Inftitute of France, &c.

ticularly diftinguifhed himfelf by his affiduity in varying the experiments. Dr. Monro wrote upon the fubject, and the National Inftitute of France publifhed an elaborate report, drawn up by a Committee compofed of feveral of its moft learned members. As however thefe works do not appear to contain any facts which materially illuftrate the nature of the galvanic influence, or lead us to form any more accurate notions refpecting its operations, we fhall in this brief fketch only notice the papers of Fabroni, of which an account may be found in the 4th Vol. of Nicholfon's Journal. He deduces from his experiments that galvanifm is intirely a chemical phenomenon; he finds that metals become oxidated when in contact with each other in circumftances where this would not take place if they were kept feparate. This idea he confirms by many obfervations and experiments, and fuppofes that when the galvanic influence is excited by the action of two metals that a chemical affinity is exerted; he does not however point out very clearly in what manner the chemical action which is exerted by the metals can be connected with the phenomena of galvanifm.

Fabroni: that galvanifm is intirely a chemical phenomenon.

History of this power in Cavallo's Electricity, and the Supplement to the Encyclopædia Britannica.

In the year 1789, Cavallo publifhed a new edition of his Electricity, and he added a good account of the principal facts in galvanifm; an ample hiftory of this fcience may be alfo found in the Supplement to the Encyclopædia Britannica.

Grand difcovery of the pile by Volta.

In this ftate our knowledge of galvanifm continued until the beginning of the year 1800, when Volta made his difcovery of the apparatus ufed called the Galvanic Pile. In the Phil. Tranf. for that year is a letter from Volta containing an account of the pile, and a detail of many curious experiments which he had performed with it.

Defcription.

This inftrument confifts of a number of circular pieces of two different metals, laid alternately upon each other, with a piece of moiftened pafteboard or fkin interpofed between each pair. The metals which anfwer the beft for this purpofe are zinc and filver, which were found to be the moft powerful in exciting the mufcular contractions in the former experiments. If the two pieces of metal which form the extremities of the pile be grafped firmly

Galvanic fhock.

in the hands previoufly moiftened, a fhock will be felt through the hands and arms, more or lefs powerful in proportion to the fize of the pile. This fhock may be repeated as long as the

the pafteboard between the metals retains its moifture. Volta conceives that this apparatus in every refpect refembles the electric organ of the torpedo and gymnotus electricus.

As foon as this difcovery became known in England, a variety of experiments were performed with the new apparatus, and many very interefting and important facts were difcovered: thefe are for the moft part detailed in Nicholfon's Journal, Vol. IV. & feq.

Carlifle and Nicholfon determine the electricity of the pile, and difcover its *chemical action, and the decompofition of water,* into hidrogen and oxigen.

In the 4th vol. p. 174, is a paper written by Mr. Nicholfon himfelf. He begins with a defcription of Volta's pile, and then relates the refults of fome experiments which were performed upon a fimilar one by himfelf and Mr. Carlifle. By ufing the revolving doubler they found that the electricity was minus in the filver end of the pile, and plus in the zinc end. Mr. Nicholfon propofed that the influence fhould be permitted to pafs from one end of the pile to the other through a tube of water; for this purpofe a divifion was made in the conducting wire, which was compofed of copper, and the two ends of it were terminated in a fmall tube of water. Immediately the wire connected with the filver end of the apparatus began to produce a gas, which was found to be hidrogen, while at the fame time the wire connected with the zinc end became oxidated. Recourfe was then had to a wire of platina which is not oxidable, and immediately gas began to be evolved from both ends. Upon examining the gafes feparately, that from the filver end was hidrogen, that from the zinc end oxigen, and they were generated nearly in the proportion requifite to produce water. This experiment, as well with refpect to its immediate effects, as with regard to the confequences which may be deduced from it, may, I think, be juftly confidered as the moft important that has occurred fince the difcovery of oxigen by Dr. Prieftley. The electric fpark was diftinctly vifible in thefe experiments.

Importance of this experiment.

Electric fpark vifible.

Cruickfhank. Effect on chemical tefts.

Mr. Cruickfhank relates his experiments, in which he made ufe of the interrupted circuit after the manner of Mr. Nicholfon; he caufed the influence to pafs firft through diftilled water tinged with litmus, and afterwards through water tinged with Brazil wood. In the firft cafe there was a rednefs produced by the zinc, and in the fecond by the filver wire. By fubmitting a portion of water for a long time to the action of the pile, a fenfible diminution was obferved in its bulk. He obferved

the

the production of hidrogen and the oxidation of the zinc end when he used a communication of copper; and when he employed a wire which was not oxidable, he obtained both hidroden and oxigen according to the observation of Mr. Nicholson.

Reduction of metals from their solution.

He permitted the influence to pass through a solution of a metallic salt, and found after some time, that the metal began to be revived at the silver end of the apparatus.

Mr. W. Henry decomposes sulphuric and nitric acids, but not the muriatic.

Mr. William Henry, of Manchester, submitted concentrated sulphuric acid to the influence of galvanism: he made use of a wire of platina. Not only the water which always enters into the composition of this acid, but also part of the acid itself, appeared to be decomposed, as the oxigen which he procured was in a larger proportion to the hidrogen obtained than is sufficient to compose water. Nitric acid was also decomposed, and the water of muriatic acid. When oximuriatic acid was subjected to its influence, the water was decomposed, and the acid was deoxigenated. Gasses do not appear to be conductors of this influence, so that Mr. Henry was not able to try its effects in decompofing muriatic acid gas. It also appears to have the power of decomposing ammonia.

Oximuriatic was deoxigenated.

Gases could not be subjected to it.

Ammonia was decomposed.

(To be continued.)

*** On account of the great Quantity of original Communications, the *Scientific News and Account of Books* is necessarily deferred.

X. Y. Z. *is informed that the derivation of chrome or chromium is from* χρωμα, color; *and was adopted by Vauquelin, because its compounds with oxigen are coloured. See Philos. Journal, quarto, II.* 444.

Mr. Robt. Jameson's Observations on the Formation of Granite.

Fig. 1.

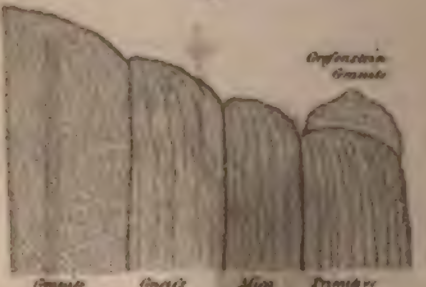
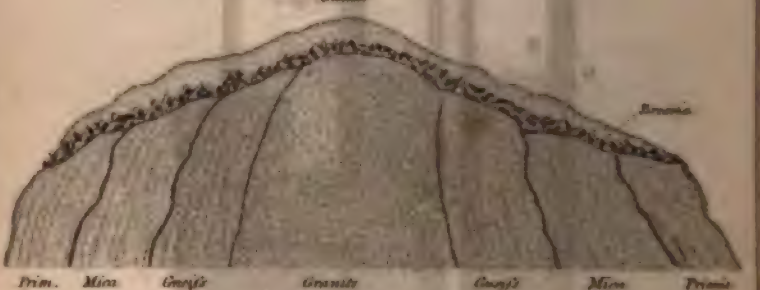
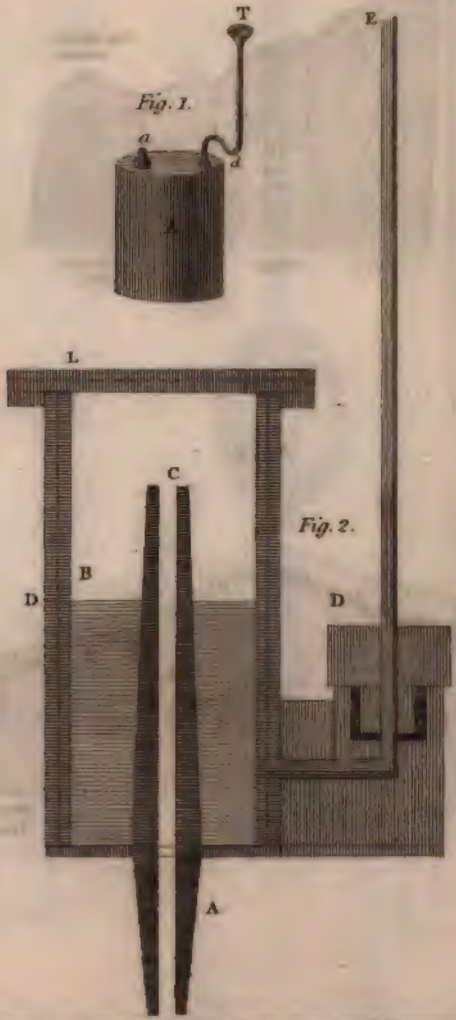


Fig. 2.



Mutlow Sc. Russell Ct.

Mr. Banks's Experiments on the velocity of effluent Air or Gas.



Atkins's Spirit Hydrometer.

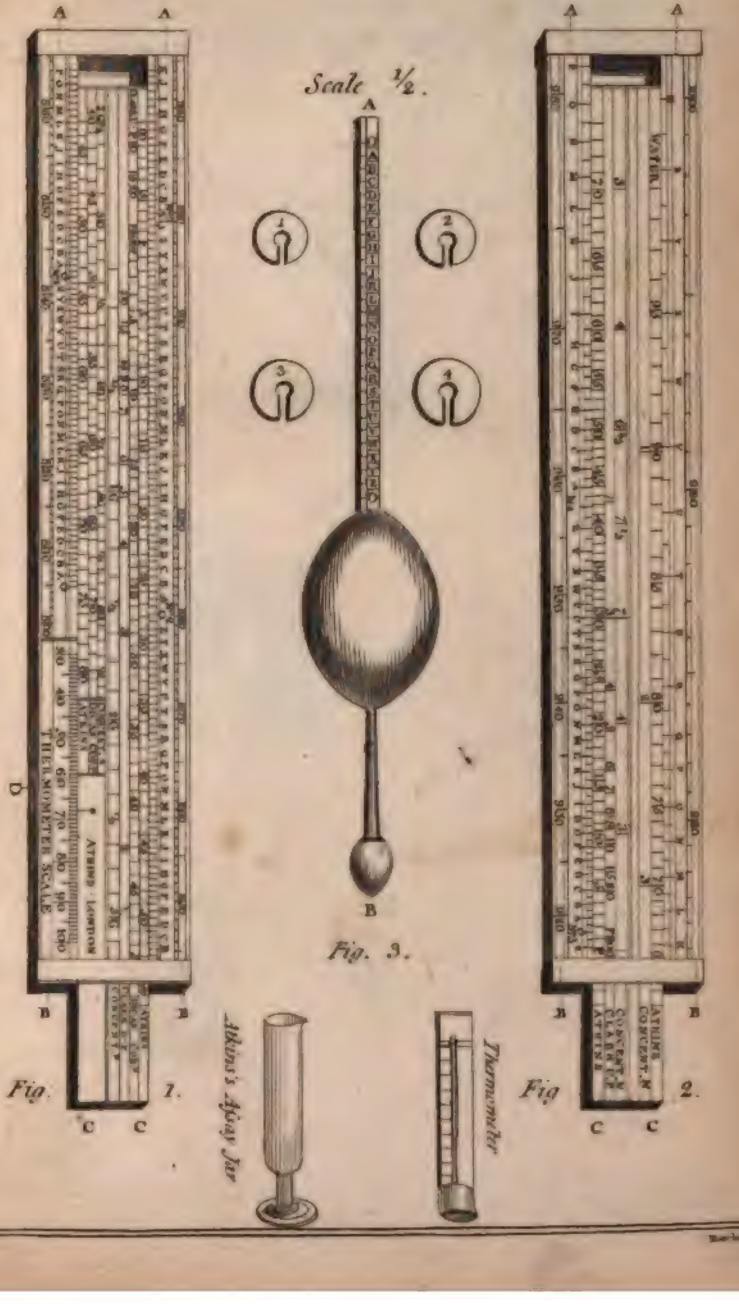


Fig. 1. Fig. 2. Fig. 3.

Compound Condensers of Electricity:
By Mr. Reed and Mr. Cuthbertson.

Fig. 1.

Fig. 2.

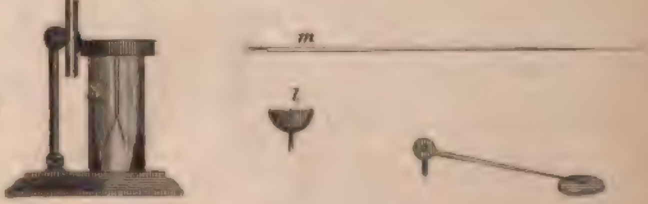


Fig. 3.

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D.

E.

3 Mineral,

Read,

Vince,

END OF THE SECOND VOLUME.

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